THE RUMFORD BICENTENNIAL

The portrait of Count Rumford on the cover was painted
around 1800 by the American painter Rembrandt Peale.
It was presented to the American Academy of Arts and
Sciences in 1872 by Joseph Harrison, Jr., to whom the
Rumford Medal had been awarded in 1871.

THE
RUMFORD BICENTENNIAL

A record of scientific and biographical papers presented at the symposium marking the two-hundredth anniversary of the birth of Benjamin Thompson, Count Rumford, held at the American Academy of Arts and Sciences in Boston on the twenty-sixth to the twenty-eighth of March, 1953.

edited by

HARLOW SHAPLEY

Chairman of the Committee on the Rumford Bicentennial
American Academy of Arts and Sciences

The Rumford Bicentennial

HARLOW SHAPLEY
Harvard University

There is no doubt about it, and it is not a savory thought that although he lived at the time of the French and American wars against tyranny, Count Rumford was instinctively a totalitarian. He was a believer in the ordered state, in the planned and efficient economy, in the detailed regulation of the common life. And while his genius was largely turned toward alleviating the discomforts of the poor and toward the enhancement of the general standard of living, it appears that he thoroughly disliked most of his fellow men, especially those of high estate in politics and science. He was in fact a multiple personality, richly equipped with contrasting virtues and vices — mostly virtues. He was a scientific genius, intermittent scoundrel, philanthropist, military wizard, practical inventor, gourmet and dietitian, international spy, architect, intellectual pioneer, ingenious cook, soldier of fortune, and one of the greatest practical servants of mankind — altogether an attractive character. If he had had the genial personality and love of his fellows exhibited by Benjamin Franklin, or the broad humanity of Leonardo da Vinci, or the sympathetic insight into human nature of Goethe, he would have ranked with them among the world's great. But such softening qualities might well have sterilized his restive genius and certainly would have prevented this poor apprentice boy-clerk out of a Boston suburb from becoming one of the near great successively in Bavaria, England, and France.

For his less commendable characteristics Rumford was chased out of New Hampshire and Massachusetts by indignant fellow citizens. He was a traitor to his own country and presumably later a traitor to England, which he twice left while under clouds of suspicion and calumny. Although he was knighted by England and by Bavaria (the Holy Roman Empire), both countries were glad to be rid of him. France liked him little for his quarrelsomeness and opportunistic operations, but greatly admired his science and technology. These and other picturesque features of Rumford's useful and unhappy life are presented in the following

pages in a charming sketch by his biographer, Professor Sanborn C. Brown.

The versatile Count's political chicanery and his marital and extramarital activities, so far as they are known and are of significance in the proper interpretation of his life and social philosophy, will be reported more fully in Brown's forthcoming full-length biography. It is my surmise that Rumford's asset to society far exceeded his detriment. It is a grim compliment that probably only the high scientific skill with which Rumford wrote his traitorous invisible-ink report to the British about the equipment of our embattled Revolutionary forebears saved him from hanging in his early twenties, many years before he made his revolutionary experiments on heat and light, and introduced lasting social reforms. But we won the War of the Revolution and should, or at least can, forgive him, wondering meanwhile if other potential scientific benefactors have had less good fortune on the fringes of the law. The great Lavoisier lost his head to the French guillotine, but that paid off in a way, since his rich widow married the dashing Count Rumford and thus helped to finance his last prolific years of brilliant scientific research.

The diverse scientific and social interests of Count Rumford, as well as his personal traits, make him a most satisfactory subject for bicentennial commemoration. The American Academy of Arts and Sciences, which owes much to the famous expatriate because of his establishment of its Rumford Premium which has honored and encouraged American scientists for more than a century, appropriately undertook a celebration. It was held on March 26, 1953, the two-hundredth anniversary of Count Rumford's birth, and on the two days following. As chairman of the bicentennial committee, I shall undertake to report here on the celebration and provide a brief introduction to the program of scientific discussions. The papers presented at the three technical sessions, or summaries of them, appear on the following pages.

Heat and light were the subjects that deeply interested Count Rumford, in addition to shady politics, militarism, and social planning and doing. In these days of great emphasis on electronics, nuclear energy, and related exciting phases of physico-chemical

science, relatively little interest is shown in the properties of heat. It is mostly a subject for engineers, not for academic scientists. A century and a half ago, however, the subject was of much theoretical and practical import. Atomic physics, spectroscopy, physiology, biological evolution, and most of the rampant modern scientific technologies had not been born. The Caloric theory, the most famous of false doctrines (after astrology and similar black arts), ruled the physical sciences and scientific philosophy. Rumford was suspicious of the Caloric theory. From suspicion he proceeded to investigations — from investigation to conviction that the hypothesis is false, although it took decades for the full acceptance of the evidence he produced and for the writing of the obituary of Caloric. His experiments and arguments were basic in slaying the persisting hypothesis that heat is a fluid without substance. The dogma was long and ingeniously defended.

The association of heat and light were as natural in Rumford's day as now. The original conditions for the award of the Rumford Premium by the American Academy of Arts and Sciences emphasize the union. They read in part as follows:

> ... that the interest [on the endowment of five thousand dollars] ... may be ... given once every second year, as a premium, to the author of the most important discovery or useful improvement, which shall be made and published by printing, or in any way made known to the public, in any part of the Continent of America, or in any of the American Islands during the preceding two years, on Heat, or on Light. ... If during any term of two years, reckoning from the last adjudication ... no new discovery or improvements should be made in any part of America relative to either of the subjects in question (Heat or Light), which, in the opinion of the Academy, shall be of sufficient importance to deserve this Premium, in that case it is my desire that this Premium may not be given, but that the value of it may be reserved, and by laying out in the purchase of additional stock in the American funds may be applied to augment the capital of this Premium.

The committees on the Rumford Award have modified the stipulations from time to time, sometimes with the help of appropriate legal decisions. For example, in practice the concept

" radiation " has been substituted for " light," and we have then reached out beyond ordinary visual light in both directions — through the red all the way to radio waves, and through the violet down to gamma radiation, even to cosmic rays. Also it was found long ago that the awarding of medals could not be justified often enough to prevent the income from the original endowment from increasing toward a sum impractical as an endowment for medals, even when the medals are large and made of both gold and silver. In consequence, with permission of the Court, the accumulated income has been for about one hundred years used on nearly four hundred occasions for grants in aid of research in the broad fields of heat and light. These grants have been used to pay for material, instruments, technical assistance, travel, and other expenses of the investigations. This aid to American science has been important over the past century, but its relative value has recently diminished because of the creation of other granting agencies and the growth of very elaborate assistance to research in heat and light from governmental sources. The number of applications to the Rumford fund has fallen off, and the accumulated income has from time to time been added to capital.

Harvard College, the beneficiary of Rumford's residuary estate under his will executed in France in 1812, made an effort in 1832 to commandeer the accrued interest of the Academy's Rumford fund. Judge Shaw of the Supreme Court of the Commonwealth of Massachusetts considered the evidence fully, noted the revised plan of the Academy for the use of the Rumford fund, and said no. He said it emphatically. And the fund that was originally five thousand dollars is now approximately one hundred thousand dollars.

By 1953 the growth of the Rumford fund and of its income had provided the agreeable circumstance that the gift made by Count Rumford in 1796 was able to carry most of the expense of the celebration of his own bicentennial. Such a practical husbanding of physical resources, as well as their use for a scientific celebration garnished with well-cooked food, would no doubt have pleased the Count very much if he could have participated. For the academicians did well by themselves at the bicentenary, both in science and in wassailing, as the following account of the ceremonies will intimate.

Rumford also endowed in the Royal Society of London an award in recognition of investigations of light and heat, and the medalists have been selected from any country. Professor Robert Williams Wood of Johns Hopkins University is the holder of both the American and British medals. S. P. Langley was also so recognized, and the elder Lord Rayleigh twice won the British award. The Royal Society's medals prior to 1900 went to many scientists who bore famous and familiar names, such as Rumford (the first award, in 1802), Davy, Fresnel, Faraday, Arago, Stokes, Pasteur, Maxwell, Kirchhoff, Tyndall, Fizeau, Angstrom, Lockyer, Janssen, Cornu, Huggins, Abney, Langley, Hertz, Röntgen, and Oliver Lodge.

Since 1900 the recipients of the medal awards from the Royal Society have been the following:

1900. Antoine H. Becquerel. *Radiation from uranium.*
1902. Charles A. Parsons. *Steam turbine.*
1904. Ernest Rutherford. *Radioactivity.*
1906. Hugh L. Callendar. *Heat experiments.*
1908. Hendrik A. Lorentz. *Optics.*
1910. Heinrich Rubens. *Long wave radiation.*
1912. Kamerlingh Onnes. *Low temperatures.*
1914. Lord Rayleigh. *Thermodynamics.*
1916. William H. Bragg. *X-ray radiation.*
1918. Charles Fabry and Alfred Perot. *Optics.*
1920. Lord Rayleigh. *High vacua.*
1922. Pieter Zeeman. *Optics.*
1924. Charles V. Boys. *Gas calorimeter.*
1926. Sir Arthur Schuster. *Optics and terrestrial magnetism.*
1928. Friedrich Paschen. *Spectroscopy.*
1930. Peter Debye. *Specific heats.*
1932. Fritz Haber. *Thermodynamics.*
1934. Wander J. de Haas. *Low temperatures.*
1936. Ernest J. Coker. *Polarized light in engineering.*
1938. Robert W. Wood. *Physical optics.*
1940. Karl M. G. Siegbahn. *X-ray spectroscopy.*
1942. Gordon M. B. Dobson. *Upper air physics.*
1944. Harry R. Ricardo. *Internal combustion engines.*
1946. Sir Alfred Egerton. *Physical chemistry in technology.*
1948. Franz E. Simon. *Temperatures near absolute zero.*
1950. Sir Frank Whittle. *Jet propulsion of aircraft.*
1952. Fritz Zernike. *Phase contrast microscopy.*

The American Academy's list of medal recipients before 1900 includes the names of Alvan Clark (telescopes), Josiah Willard

Gibbs (thermodynamics), H. A. Rowland (wavelengths), S. P. Langley (solar radiation), A. A. Michelson (velocity of light), E. C. Pickering (stellar photometry), and Thomas A. Edison (electric lighting).* The full list since 1900 is as follows:

1900. Carl Barus. *Various researches in heat.*
1901. Elihu Thomson. *Electric welding and lighting.*
1902. George E. Hale. *Solar and stellar physics.*
1904. Ernest F. Nichols. *Radiation pressure.*
1907. Edward G. Acheson. *Electric furnace researches.*
1909. Robert W. Wood. *Optical properties of metallic vapors.*
1910. Charles G. Curtis. *Improvements in steam turbines.*
1911. James M. Crafts. *High temperature thermometry.*
1912. Frederic E. Ives. *Color photography.*
1913. Joel Stebbins. *Selenium cell photometry.*
1914. William D. Coolidge. *Invention of ductile tungsten.*
1915. Charles G. Abbot. *Researches on solar radiation.*
1917. Percy W. Bridgman. *Thermodynamics at high pressures.*
1918. Theodore Lyman. *Short wavelength radiation.*
1920. Irving Langmuir. *Thermionics and allied phenomena.*
1925. Henry N. Russell. *Researches in stellar radiation.*
1926. Arthur H. Compton. *Röntgen rays.*
1928. Edward L. Nichols. *Spectrophotometry.*
1930. John S. Plaskett. *Stellar spectroscopy.*
1931. Karl T. Compton. *Thermionics and spectroscopy.*
1933. Harlow Shapley. *Luminosity of stars and galaxies.*
1937. William W. Coblentz. *Heat and light technology.*
1939. George R. Harrison. *Improvements in spectroscopic techniques.*
1941. Vladimir K. Zworykin. *Invention of television devices.*
1943. C. E. Kenneth Mees. *Science of photography.*
1945. Edwin H. Land. *Polarized light and photography.*
1947. E. Newton Harvey. *Nature of bioluminescence.*
1949. Ira S. Bowen. *Interpretation of nebulium.*
1951. Herbert E. Ives. *Researches in optics.*
1953. Enrico Fermi. *Neutrons and mesons.*
1953. Willis E. Lamb, Jr. *Excited hydrogen atoms.*
1953. Lars Onsager. *Irreversible processes.*

The various Rumford committees can probably explain why the names of Curie, Planck, and Einstein do not appear in the Royal Society's list, or the last named in the list of the American Academy.

*A complete list of recipients of medal awards prior to 1950 by both the Royal Society and the American Academy, together with a listing of Rumford grants for research, is given in the Academy's booklet, "The Rumford Fund" (1950).

The Rumford Celebration began with the birthday banquet in the Great Hall of the Harvard Club of Boston. The program of the evening is reproduced on pages 260-263. The illustrated bill of fare was compiled by Professor Brown, under whose direction were also prepared the models of many of the inventions and contrivances of Count Rumford which were on display in the anteroom. Pictures of some of this equipment are reproduced in the bill of fare and more extensively in Professor Brown's biographical sketch.

The Rumford Medalists among the guests of honor, listed on page 263, most of whom were able to be present, were with one exception introduced by the present writer, who read for each the citation of the reasons for his selection, as engraved on the respective medals; the other guests were introduced by the President of the Academy, Edwin H. Land. The banquet, the Bicentennial Lecture, and the " pleasing flow of spirits " that graced the whole occasion, brought substantial satisfaction to the academicians and their guests.

The program of three scientific symposia and the evening session for the presentation of three Bicentennial Rumford Medals is reproduced on page 264. As is usual, the greatest value of the conference came from the informal out-of-meeting conversations of the participants. The conferees included not only those mentioned on the program but a considerable number of advanced students from the Boston community and specialists in heat and light from educational institutions and scientific industries. The Ford Foundation generously arranged to cover the travelling expenses of three special guests from abroad. To the executive officer of the Academy, Mr. Ralph Burhoe, and his staff, the committees are deeply indebted for effective management of all details of the Celebration.

Of the three symposia on Recent Developments, only the first on Thermodynamics actually dealt with scientific matters close to the interests of Count Rumford. Atomic spectroscopy and nuclear physics were for him non-existent as fields of research and providers of material for the subsequent rich technologies. The three fields are now so widely dispersed that the contributions collected here do not form a unit in any sense, except that they are all in the areas of the physical sciences and all contributions are concerned with

BICENTENNIAL
Birthday Banquet

IN HONOR OF

Benjamin Thompſon, *Count Rumford*

born in Woburn, Maſſachuſetts

March 26, 1753

American Academy of Arts and Sciences

*at the Harvard Club of Boſton*

March 26, 1953

BILL OF FARE

Defcribed and illuftrated by Benjamin Thompfon

" PEACE OF MIND, which is as effential to contentment and
happinefs as it is to virtue, depends much upon order
and regularity in the common affairs of life; and in no
cafe are order and method more neceffary to happinefs
(and confequently to virtue) than in that where the
prefervation of health is connected with the fatiffying
of hunger, an appetite whofe cravings are fometimes as
inordinate as they are infatiable." (1796)

"ONE PORTION of foup:

|		
of pearl barley	0	$\frac{1129}{1200}$ oz.
of peas	0	$\frac{1050}{1200}$ oz.
of potatoes	3	$\frac{84}{1200}$ oz.
of bread	0	$\frac{1114}{1200}$ oz.
of water	13	$\frac{127}{1200}$ oz."

(1795)
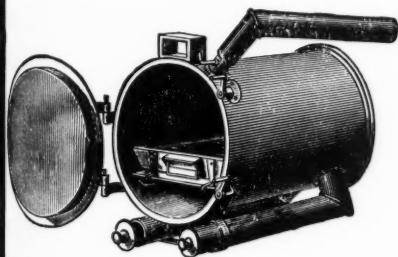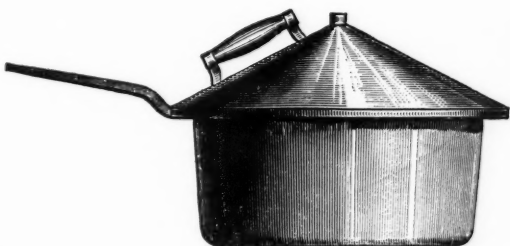
" HOWEVER BOLD the affertion may
appear, I will venture to affirm that
meat of every kind, without any ex-
ception, roafted in a roafter, is better
tafted, higher flavoured, and much
more juicy and delicate than when
roafted on a fpit before an open
fire." (1799)

" THERE IS NO ARTICLE uſed as food of which a greater variety of well-taſted and wholeſome diſhes may be prepared than of potatoes." (1796)
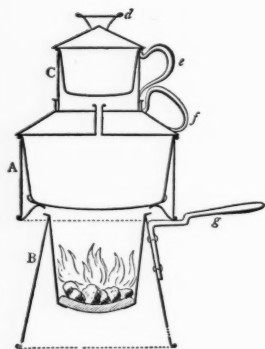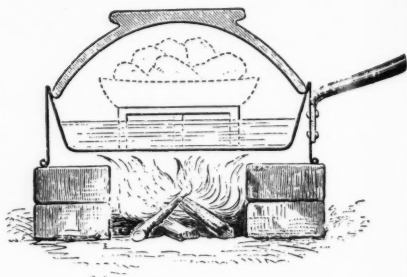
" WHAT IN AMERICA is called a plain Indian pudding certainly holds the firſt place and can hardly fail to be much liked by thoſe who will be perſuaded to try it." (1796)

" AMONG THE NUMEROUS LUXURIES of the table unknown to our forefathers, which have been imported into Europe in modern times, COFFEE may be conſidered as one of the moſt valuable. Its taſte is very agreeable, and its flavour un‣commonly ſo; but its principle excellence depends on its ſalubrity and on its exhilarating quality. It excites cheerfulneſs without intoxication, and the pleaſing flow of ſpirits which it occaſions laſts many hours, and is never followed by ſad-ness, languor, or debility. It diffuſes over the whole frame a glow of health, and a ſenſe of eaſe and well-being which is exceedingly de-lightful. Exiſtence is felt to be a poſitive enjoyment, and the mental powers are awakened and rendered uncommonly active." (1812)
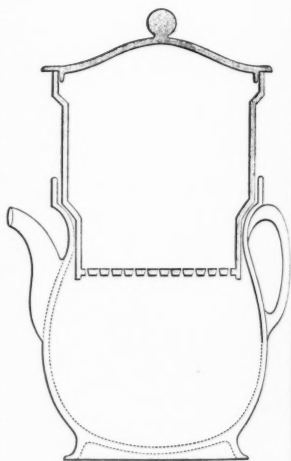
Guefts of the Academy

Rumford Medalifts of the
American Academy of Arts and Sciences

1909	Robert Williams Wood
1913	Joel Stebbins
1914	William David Coolidge
1915	Charles Greeley Abbot
1917	Percy Williams Bridgman
1918	Theodore Lyman
1920	Irving Langmuir
1925	Henry Norris Ruffell
1926	Arthur Holly Compton
1931	Karl Taylor Compton
1933	Harlow Shapley
1937	William Weber Coblentz
1939	George Ruffell Harrifon
1941	Vladimir Kofma Zworykin
1943	Charles Edward Kenneth Mees
1945	Edwin Herbert Land
1947	Edmund Newton Harvey
1949	Ira Sprague Bowen
1951	Herbert E. Ives
1953	Enrico Fermi
1953	Willis Eugene Lamb, Jr.
1953	Lars Onfager

Rumford Medalifts of the Royal Society

1930	Peter Debye
1940	Karl Manne Georg Siegbahn
1946	Alfred Egerton
1948	Franz Eugen Simon

Rumford Profeffor of Harvard Univerfity
Emory Leon Chaffee

Reprefentative of the Rumford Hiftorical Society
Richard C. Johnfon

Rumford Bicentennial Lecture
COUNT RUMFORD — *Phyficift and Technologift*
by Sanborn Conner Brown

RUMFORD BICENTENNIAL SYMPOSIA AND AWARDS
March 27 and 28, 1953

Symposium A
RECENT DEVELOPMENTS IN THERMODYNAMICS
Academy House

Friday Morning, March 27, 9.30 o'clock

Chairman: JOSEPH H. KEENAN, Massachusetts Institute of Technology

Vice-Chairman: MARK W. ZEMANSKY, City College of New York

P. W. BRIDGMAN, Harvard University
Reflections on Thermodynamics

SIR ALFRED EGERTON, Imperial College of Science and Technology, London
Management of Flame

LARS ONSAGER, Yale University
Reciprocal Relations in Irreversible Processes

FRANZ E. SIMON, The Clarendon Laboratory, Oxford
Helium and the Range of Stability of the Solid State

Awards
PRESENTATION OF THE RUMFORD BICENTENNIAL MEDALS
Academy House

Friday Evening, March 27, 8.15 o'clock

Presiding: EDWIN H. LAND, President, American Academy of Arts and Sciences

Master of Ceremonies: GEORGE R. HARRISON, Chairman, Rumford Committee, American Academy of Arts and Sciences

To ENRICO FERMI, University of Chicago
Introduced by PETER DEBYE, Cornell University

To WILLIS E. LAMB, JR., Stanford University
Introduced by NORMAN F. RAMSEY, Harvard University

To LARS ONSAGER, Yale University
Introduced by JOHN G. KIRKWOOD, Yale University

Reception for Rumford Medalists

Symposium B
RECENT DEVELOPMENTS IN ATOMIC SPECTROSCOPY
Academy House

Friday Afternoon, March 27, 2.30 o'clock

Chairman: DONALD F. HORNIG, Brown University

Vice-Chairman: ROBERT V. POUND, Harvard University

MARTIN DEUTSCH, Massachusetts Institute of Technology
Positronium

WILLIS E. LAMB, JR., Stanford University
Excited Hydrogen Atoms

EDWARD M. PURCELL, Harvard University
Line Spectra in Radio Astronomy

I. I. RABI, Columbia University
Molecular Beam Radiofrequency Methods for the Study of Excited States of Atoms

Symposium C
RECENT DEVELOPMENTS IN NUCLEAR PHYSICS
Academy House

Saturday Morning, March 28, 9.30 o'clock

Chairman: EDWIN B. WILSON, Office of Naval Research

Vice-Chairman: WALTER SELOVE, Harvard University

ENRICO FERMI, University of Chicago
Meson Physics

J. ROBERT OPPENHEIMER, Institute for Advanced Research, Princeton, N. J.
Recent Progress in the Understanding of Nuclear Forces

MANNE SIEGBAHN, Nobel Institutet för Fysik, Stockholm
Nuclear Spectroscopy

VICTOR F. WEISSKOPF, Massachusetts Institute of Technology
Problems of Nuclear Structure

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new developments. Only the remarkable Reflections on Thermodynamics by Bridgman linked the historical past with the somewhat ragged thermodynamical concepts of the present. In fact, in several contributions, such as those by Egerton, Deutsch, Purcell, Oppenheimer, and Weisskopf, the science of the future came under constructive scrutiny more than did the preliminaries of the past.

Several of the reports printed in the following pages fall short of a full account of the contributions as presented in the symposia. A few of the communications came from exceedingly busy scientists who talked effectively but could not write papers or summaries for this number of the Proceedings of the Academy. Presumably their contributions will be fully presented later in the technical literature of their fields. The papers that are here included commemorate fittingly the New Englander of two centuries ago who honored our country, and other countries as well, by promoting the basic sciences that Heat our curiosity and Light our meditations on the nature of Nature's most important obscurities.

Rumford Bicentennial Lecture

Count Rumford —
Physicist and Technologist *

SANBORN C. BROWN
Massachusetts Institute of Technology

Two hundred years ago, on March 26, 1753, a son was born to simple farmer folk in Woburn, Massachusetts. This child, Benjamin Thompson, seemed quite an average boy, and like all youngsters curious about scientific things, he played with electricity and fireworks as our present-day small boys play with electric toys and chemistry sets. His father, however, had died when he was only two, and by the time he was thirteen Ben was apprenticed as a clerk to an importer in Salem.

The importing business was not very prosperous in the 1760's. Mr. John Appleton's business was no exception, so that when young Benjamin was taken so ill that he was sent home for his mother's care, Mr. Appleton asked that he not return on his recovery.

Thompson's next job was with a dry-goods merchant in Boston who kept shop in the building that is now occupied by the Old Union Oyster House. Here he found that he was rather more interested in playing with gunpowder and inventing rockets and fireworks than he was in selling cloth and dry goods. When one of his gunpowder experiments blew up violently in his face, his employer, Mr. Samuel Capon, suggested forcefully that he look for employment elsewhere.

His scientific bent having by this time been recognized, he took up the study of medicine with a Dr. John Hay of Woburn. He seems to have really applied himself to the study of medicine, though one must confess that he seems to have spent more time in describing love potions in his notebooks with all the O's in the form of little hearts than in a serious effort to become a doctor. However it was while he was with Dr. Hay that he submitted his

* This paper was also published in the *American Scientist* of January, 1954.

first scientific paper for publication. This article, describing an abnormal child born in Woburn in 1771, was duly recorded as received by the American Philosophical Society of Philadelphia, but was never actually published. As a matter of fact, we know the details of this paper only by the minute description and sketch of this monster in Thompson's notebooks. Thompson was clearly intrigued by the more sensational aspects of his medical studies, but apparently found the work lacking in permanent appeal. In 1772 he left Woburn to teach school in Bradford, Massachusetts, where he found a guide and tutor in the local minister, the Reverend Samuel Williams.

One might say, parenthetically, a brief word about the checkered career of the Reverend Dr. Williams. An eminent fellow and charter member of the American Academy of Arts and Sciences, Dr. Williams became, some years later, Hollis Professor of Mathematics and Natural Philosophy at Harvard, a post which he held until he was accused of embezzling Harvard funds and was summarily dismissed by the overseers of that institution. To escape the public clamor, he fled to the Northern Wilderness, to found subsequently the University of Vermont.

But to return to our main subject, most of Benjamin Thompson's formal study seems to have been derived from contact with the Reverend Samuel Williams, and this was not restricted to the more conventional disciplines of mechanics, heat and light. He built Dr. Williams an electrical machine, and under his sponsorship sent another paper to the American Philosophical Society describing a " Remarkable Aurora Borealis " observed at Bradford in 1772. This aurora seemed more remarkable to Thompson than it did to the editors of the American Philosophical Society, who filed it with the comment: "Anyone living in our northern climes might observe fifty such displays in a year."

School teaching in Bradford failed to satisfy Thompson's need for something greater, or at least different, and in the summer of 1772 he departed to take a similar job in Concord, New Hampshire. Concord was populated mainly by families moving north from Woburn, and it was therefore not surprising that he came to Concord with the sponsorship of Concord's pastor, the Reverend Timothy Walker, himself an old Woburn native. It soon became evident that the patronage of the Reverend Mr. Walker was not

the only attraction in the Walker home, for Mr. Walker had a daughter, Sarah by name, who had just recently been widowed. The death of her husband, Colonel Benjamin Rolfe, left Sarah the richest landowner in Concord.

It took Thompson just four months to woo and wed the Widow Rolfe, fourteen years his senior. Since this marriage made him the proprietor of two-thirds of the land in the town, he found the job of schoolmaster somewhat superfluous. He wrote to his friends at the time that he was seriously taking up the science of husbandry, sending to England for great supplies of seeds and grains. We have records of Thompson's claims in court to lands whose titles had been clouded by the long legal dispute over the boundaries of the towns of Bow and Rumford. (Rumford's name had been changed to " Concord " to celebrate the fixing of the boundary lines of these formerly overlapping townships.)

Thompson's marriage to Sarah Rolfe had done more for his fortunes than just to make him wealthy. It hurtled him into the middle of the fashionable society surrounding the Royal Governor, John Wentworth, in Portsmouth. In this exciting environment Thompson found his true milieu. How he so successfully curried the favor of the Royal Governor will probably never be known, but curry it he certainly did. Within two years the lowly-born country schoolmaster from Bradford donned the handsome uniform of a major in the 15th Regiment of New Hampshire Militia. He was not quite twenty-one years old.

It must be confessed that the 15th Regiment of Militia was hardly more than a paper regiment, consisting as it did at its fullest of five commissioned officers, twelve non-commissioned officers, and no privates, but an intimate connection with any such organization was scarcely calculated to make Thompson popular with the revolutionary-minded patriots of Concord. To make matters worse, Thompson was strongly suspected of working actively for the British by hiring disguised British regulars to round up British deserters in the vicinity and ship them back to General Gage in Boston. It was hard to prove in 1774, but we know now that this was precisely what Thompson was doing.

The patriots of Concord did more than talk. After Thompson had been hauled before the local Committee of Safety, who could find no definite proof of disloyalty, the populace decided to take

matters into their own hands. Wisdom proved the better part of valor, and Thompson galloped abruptly away into the night on his brother-in-law's best horse to avoid the interesting experience of being tarred and feathered and ridden out of town on a rail by the irate citizens of Concord.

Back in his home town of Woburn, Thompson made no bones about his loyalty to the British Crown nor his feeling of injured pride at his treatment at the hands of the New Hampshire patriots. There still exists a remarkable record of his service to the British cause in the form of a secret-ink letter full of military information which he passed through the American lines to General Gage three weeks after the Battle of Lexington and Concord. If the letter had been intercepted, its author could easily have ended his career at the end of a rope, but because of the excellence of its technical chemistry, the message was as secure as any espionage activity can be. Like his erstwhile friends in Concord, the citizens of Woburn suspected that Thompson was more than an innocent bystander to the gathering revolution, but, as in Concord, the evidence was lacking. He was brought before the Massachusetts Committee of Correspondence, accused of what we now know he was doing, but no concrete proof could be produced, and he was released. He found it expedient, however, to leave Woburn shortly thereafter to join openly the British Loyalists in Boston.

Thompson's powers of observation served him well. In Boston his first act was to write a very detailed report on the " State of the Rebel Army," which outlined not only the state of morale, health, and ability of the American Army, but gave details of shipbuilding in Cambridge, the size and location of ammunition stores in the surrounding areas, their geographic vulnerability, and even how many guards were assigned to each. As a record of detailed and cogent reporting, it is a remarkable document.

How long Benjamin Thompson stayed in Boston before going to London we do not accurately know. We do know that he came very early to the attention of the Secretary for the Colonies, Lord George Germain, and rose very rapidly under his patronage. He was made Secretary of the Province of Georgia in December 1775 and had become Undersecretary of the Northern Department by September 1780. Concurrent with his meteoric rise in the British government he found more and more time for scientific

experimentation, primarily along military lines. He began at this time a long series of investigations on the force of fired gunpowder, and read his first paper on this subject before the Royal Society of London (of which he had become a member in 1779) in March 1781.

His interest in the force of fired gunpowder was naturally directed toward increasing the range of cannon balls fired from actual cannon. Since these measurements were difficult to do on land, Lord George Germain arranged for him to go on a cruise with Admiral Hardy's fleet to continue his studies under the more favorable conditions of naval bombardment. Not only did Thompson carry out experiments on the range of naval guns but he was profoundly shocked by the ridiculous inadequacy of marine signals. The event which really set him to thinking about the problem began with the sighting of a sail on the horizon which was thought to be part of the French fleet. The British admiral ran up what he considered to be an order for the ships to form immediately into the line of battle. To the consternation of those on the flagship, the response to the signal was a lowering of sails and the putting to sea of small cutters toward the Admiral's ship. What was supposed to be the order for line of battle turned out to be the signal for the weekly pay distribution!

Thompson worked out a system of signals that was apparently very good and was accepted by the British Admiralty, but disappeared because of security reasons, and we now know nothing about it.

All the evidence points to the fact that Thompson's interest was not solely connected with naval gunnery and signaling. A few months after his return to his office in London, a French spy by the name of La Motte was tried on charges of espionage on the British navy. At the trial the identity of a " person in a certain office " was not disclosed, but the gossip of the day pointed a finger at Benjamin Thompson, suggesting further that his powerful benefactor was shielding his identity. Whatever the precise facts in the case may be, we know that La Motte was publicly drawn and quartered at a well-attended and most impressive ceremony, and Thompson abruptly sailed for America as a lieutenant colonel in command of the yet-to-be-raised Loyalist regiment of the King's American Dragoons.

Colonel Thompson's executive ability was put to a very successful test in his collecting out of nowhere of a regiment of horsemen, raising it to full complement, and then retiring for the winter of 1783 upon the town of Huntington, Long Island. Unfortunately the populace of Huntington recalled nothing but evil of that winter. Thompson stabled his horses in the Presbyterian church overlooking the town. He pitched his own tent over the grave of the recently deceased minister, an ardent patriot, so that (according to his own testimony) he " could tread on the damn Yankee's head whenever he came in and out." He ordered his soldiers to make ovens of the gravestones so that they could distribute through the town loaves of bread upon whose lower crust the inverse inscriptions of deceased citizens were clearly legible.

At the close of the Revolution, Thompson returned to London just long enough to secure his government pension as a colonel on half pay before setting out as a soldier of fortune in Europe. After suitable negotiations he was invited to Bavaria as Inspector General of the Artillery and Aide-de-Camp to the Elector of Bavaria. He returned briefly to London to be knighted by George III so that his British rank would be commensurate with his Bavarian position, and finally settled down in Munich to his most profitable era of science and public administration.

Sir Benjamin was primarily a military man, and, as one would expect, many of his scientific investigations followed this direction. Because of his fundamental interest in heat and his belief that there existed some intimate relation between the force of gunpowder explosion and the heat produced, his studies in this field were extensive.

The standard method of testing the strength of gunpowder in his day was to fire cannon with various samples of powder under standard conditions, measuring the range of the shot. Since cannon were fired by powder trains and cannon balls fit loosely into the barrels of the guns, this method was far from a quantitative measurement. Thompson designed a device, shown in Fig. I, which became standard for many years thereafter.* Gunpowder was enclosed in a tightly closed bomb and fired into a closed system by a red-hot ball from the outside. The quantity of gunpowder was

*All figures in this paper are taken from Count Rumford's published papers, except the photographs in Figures II and IX.
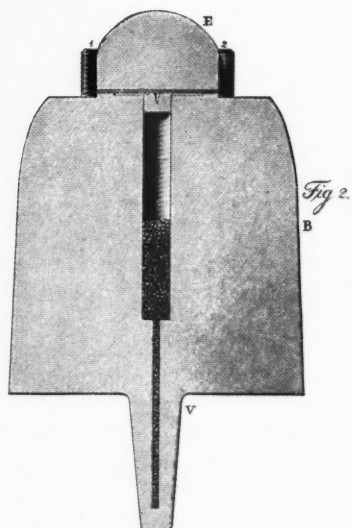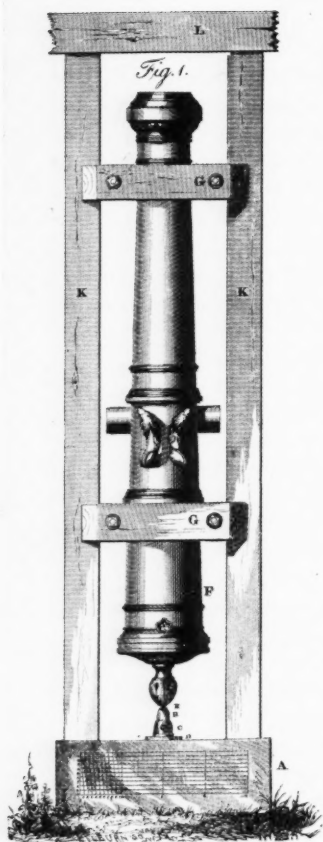
FIGURE I

adjusted so that the explosion would barely lift the bomb's tight-fitting cover, held in place by the weight of a four-ton cannon.

Having always been familiar with the heat evolved in firing guns and cannon, Thompson, even as a young man, was keenly interested in theories of heat. The generally accepted theory of the day was that heat was a subtle fluid called caloric. Thompson supported an opposing theory that heat was a form of energy, and devised experiments throughout his career to disprove the existence of caloric.

Thompson reported the most famous of his anti-caloric efforts in "An Inquiry Concerning the Source of Heat which is Excited by Friction." In this experiment he showed that the heat generated in the process of boring cannon was a definite, measurable quantity which did not diminish as long as the experiment was continued. He measured the heat produced by his cannon boring by measuring the time necessary for this heat to boil a given quantity of water. Fig. II shows the form his apparatus took. His conclusion was " that anything which any *insulated* body . . . can furnish *without limitation*, cannot possibly be a *material substance:* and it appears to me to be extremely difficult . . . to form any distinct idea of anything, capable of being excited and communicated in these experiments, except it be MOTION."

The expansion of a body on being heated was easy to explain on the material theory by the idea that the entering caloric took up space. Any demonstration which could show a body expanding on being cooled would be a clumsy problem for the caloricist. One such case is the behavior of water below 4°C, and Thompson carried out a long and careful series of experiments to show that water had its maximum density at 4°C, and would expand when cooled from that temperature downwards.

The caloric theory maintained that heat was a fluid which spread from hot objects to cooler ones and seemed to grow in extent because it was inherently self-repulsive. Caloricists felt that a lens could not produce an infinite temperature at its focus because the self-repulsive character of the caloric fluid should keep it from concentrating too much. To study the effect of converging and diverging rays, Thompson designed an instrument to measure the amount of heat in sun rays passing through lenses. His illustration of the receiver of his apparatus is shown in Fig. III. He pointed
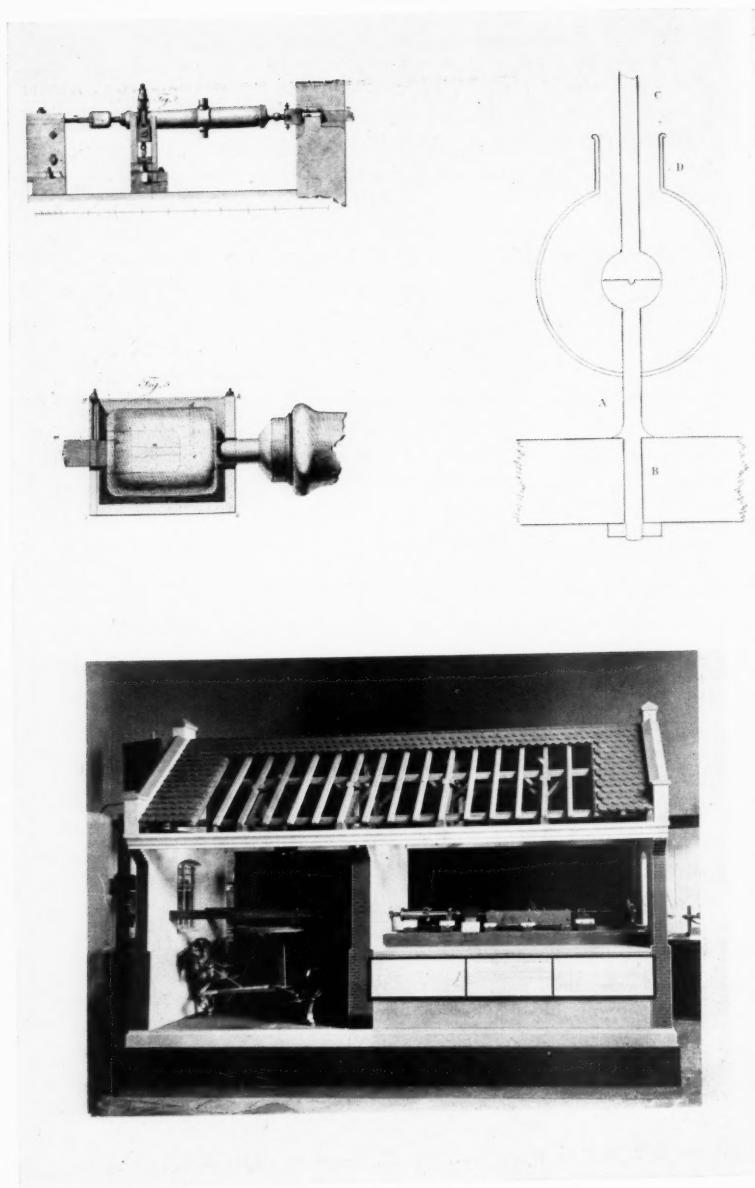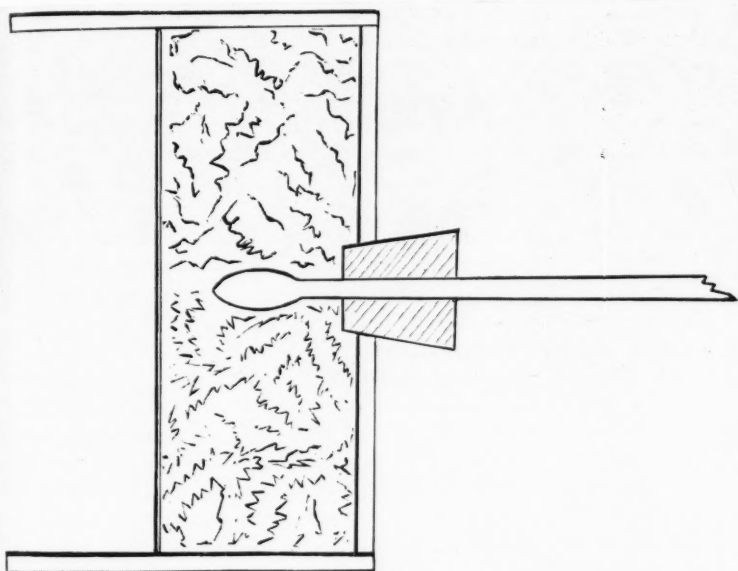
FIGURE II

FIGURE III

his instrument toward the sun and measured the relative tempera-
ture rise of water contained in two metal boxes when lenses of the
same focal lengths were located at different distances from the
surface of the boxes. He showed that the amounts of heat passed
by two lenses of the same diameter is the same whether the rays
are parallel, diverging, or converging.

Caloric was considered to be conducted along a solid body by
being attracted by the particles of the body deficient in the caloric
fluid. Since this attraction seemed akin to a gravitational force,
the caloricists considered that heat should have weight. Sir Ben-
jamin disproved this theory by a most ingenious experiment. He
very carefully balanced two identical vials containing alcohol and
mercury respectively. He showed that they stayed in balance over
a temperature variation of over 50°F in spite of the fact that because
of the difference in the specific heat of the bodies, the quantity of

heat taken up by the mercury was very much greater than that absorbed by the alcohol.

Thompson carried out a long series of investigations on the " Propagation of Heat in Various Substances." He showed that his observations warranted the conclusion that in a single substance the conducting power increased with the compactness of that structure, as predicted by the caloric theory. Carrying the caloric theory of conduction to its logical conclusion, conduction through a vacuum should not occur since no attractive material particles are present. Thompson was the first to prove that this prediction of the caloric theory was false because he could demonstrate heat passing through a vacuum. Several models of apparatus designed to show this effect are illustrated in Fig. IV.

Although Thompson spent the greatest part of his scientific career trying to disprove the caloric theory, the energy theory of heat was not generally accepted for another sixty to eighty years. The reason for this appears to be that no general theory of con- servation was evident in the embryonic theories of heat as a mode of motion. The conservation of caloric was a cornerstone of the material theory and therefore fitted into an over-all concept of nature. Lacking such a firm base, the energy theory of heat was not firmly established until the conservation of energy was made acceptable much later by Mayer and Helmholtz.

The failure of the natural philosophers of the day to accept his theoretical proofs of the energy theory of heat did not particularly disturb Sir Benjamin. He was fundamentally more interested in technological improvements than in pure theory, a point well illus- trated by his statement: " I have taken particular care cautiously to avoid bewildering myself in abstruse speculations. I have often thought that I should be perfectly satisfied could I but obtain the exercise of the *authority* of a magician, even though I should not be permitted to know *how* the obedient spirits I should call up performed their business. I can conceive of no delight like that of detecting and calling forth into action the hidden powers of nature! — of binding the Elements in chains, and delivering them over the willing slaves of Man! Could I have had my choice, I should much rather have been the Inventor of a water-mill, than the Absolute Sovereign and Law-giver of the whole Human Race."

In social and political organization Thompson was a rigid ad-

Fig. 1

Fig. 2

Fig. 3

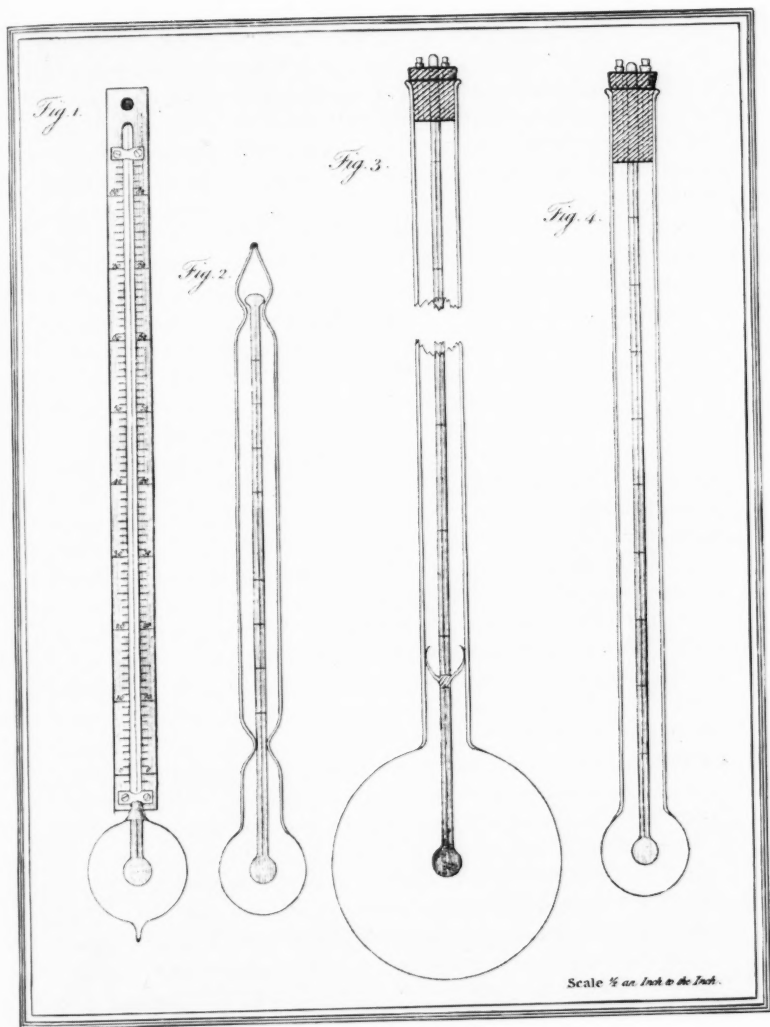Fig. 4

Scale ½ an Inch to the Inch.

FIGURE IV

herent to order and efficiency, and throughout his regime in Bavaria these principles dominated all his thinking. For reasons of European politics, the Bavarian Army had to be numerically large, but it was so poorly organized that it could not be used as a fighting unit. The soldiers had very little to do, and hence morale was sadly lacking. One of Thompson's first moves was to set up a program in all its essential features like the late President Franklin Roosevelt's W. P. A. He put the soldiers busily to work building roads and public buildings as well as parks and gardens. He attached to each military establishment a garden where the soldiers were required to raise their own food, and he tried to teach them the elements of scientific farming so that on their return to civilian life improved methods of agriculture would be spread throughout the country. The most elaborate of these projects, the English Gardens in Munich, for which Thompson appropriated the Elector of Bavaria's private deer sanctuary, is still one of the attractions of the city of Munich.

His principle of order was profoundly shocked by the plague of beggars which almost dominated the operation of the city of Munich. With characteristic vigor and directness, Thompson arrested all the beggars in the city, put them in a great prison-like " House of Industry," and set them all to work making shoes and clothes for the army. In his houses of industry where men, women, and children slaved fourteen hours a day, he organized a public school which all these children were required to attend. A hot-lunch program was provided in this school. This state-administered educational program is often considered the forerunner of our present public-school system.

Some of his institutions were objects of great amazement to his contemporaries. One of these is described by the famous early American statesman, Gouverneur Morris, who visited Munich in 1798: " We go on to the house fitting up, under Count Rumford's direction. This house was built for ladies to live in privately, and is the most superb building in Munich. The idea is the most extraordinary that I ever remember to have met with. It was further intended for the education of those young scions of nobility which had been furtively taken from the noble stock. In England this would be called a strong legislative declaration of unchasteness."

To maintain the maximum of economy and efficiency for all his

military organizations and work houses, Thompson energetically applied his investigative genius to the sciences of nutrition, the economy of fuel and cooking, the science of illumination and clothing.

To feed the maximum number of people with the highest efficiency, Sir Benjamin experimented with many types of nutritious soups, and the so-called Rumford Soup which he finally developed is still served by that name in Europe. He introduced the potato into Bavaria and incorporated it into all of his recommended menus. He studied heats of combustion of the common cooking fuels and then designed stoves, roasters, and pots and pans to extract the maximum amount of heat from the fuel. His stoves were the antecedents of our present kitchen range, the evolution of which is illustrated pictorially in Fig. V. The first picture shows the open range in common use, which was installed in the opening of an ordinary fireplace. The next picture illustrates a Rumford register stove, in which the fire playing on the bottoms of the kettles could be regulated by a register. The next figure is his plan for a complete kitchen stove unit. Rumford Roasters were famous for many years and are still to be found occasionally in old New England houses. His drawings of this roasting oven are shown in the last two pictures. Fig. VI is a composite illustration of a number of Rumford designs for pots and pans to fit individual stoves. The first row shows the construction of various utensils. The second row shows what some of them looked like from the outside. Various stove constructions are given in the third row, and combinations are illustrated in the last row. The final picture of this row shows Rumford's design of what we know today as a " fireless cooker."

Thompson tells us that he spent fifteen years trying to make a truly superior cup of coffee. All this effort resulted in the " drip " coffee maker. Fig. VII shows various designs of his coffee pots. The first two pictures are his drawings of general purpose coffee makers. The second picture shows inserts of varying radius for different amounts of coffee, since he believed that the water should always pass through the same standard thickness of coffee. Coffee urns for large numbers of people are also illustrated. He designed the single cup device " for very poor persons who cannot afford to buy a coffee-pot."
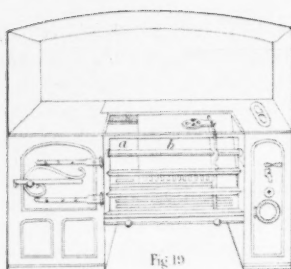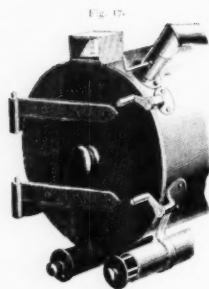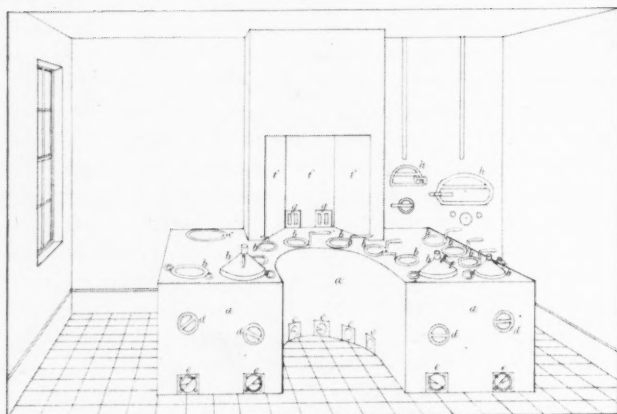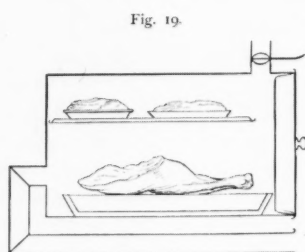
Fig 19

Fig. 48.

Fig 1

Fig. 17.

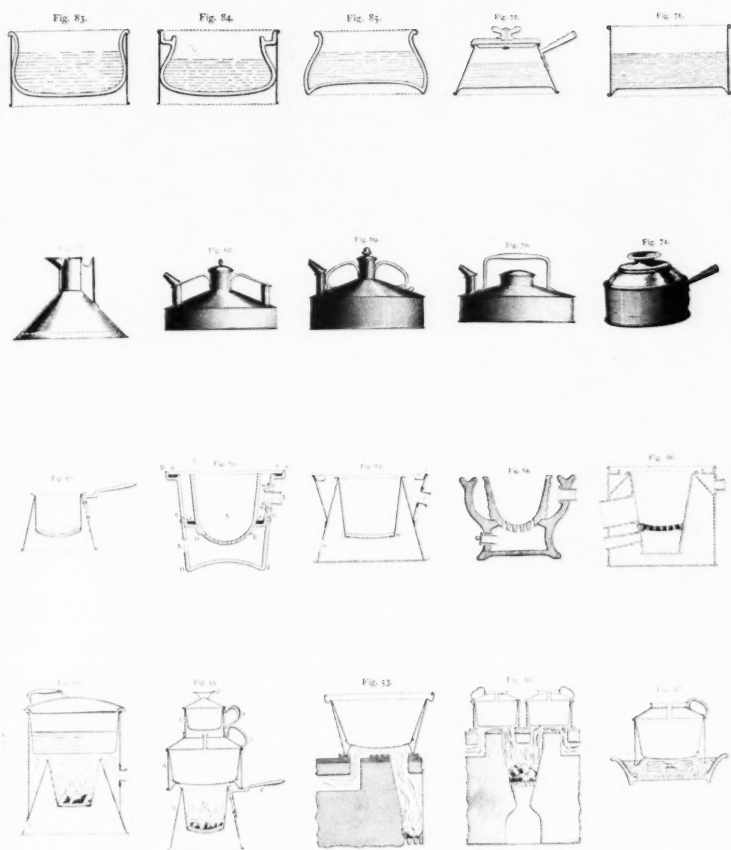Fig. 19.

FIGURE V
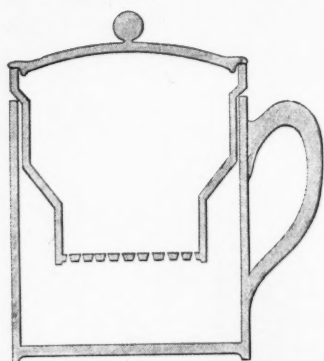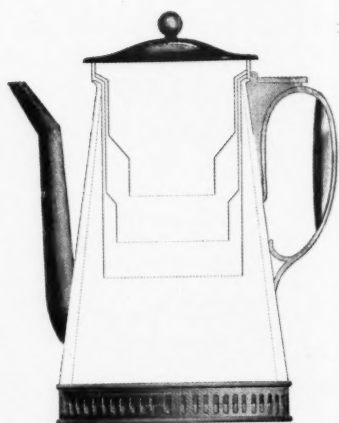
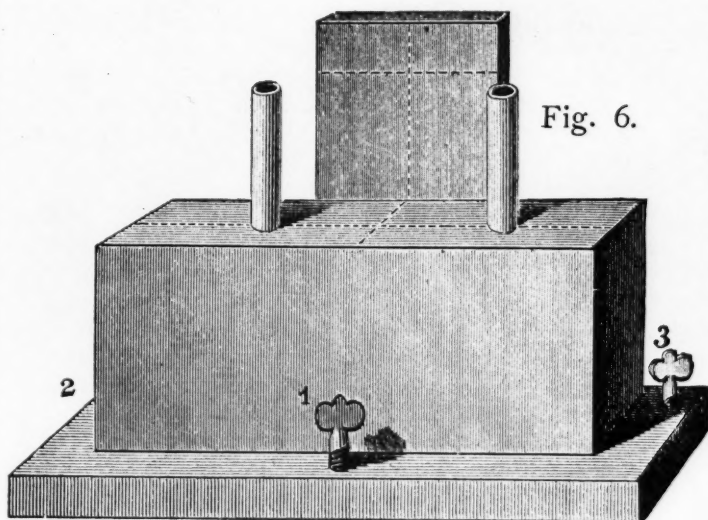Fig. 83. Fig. 84. Fig. 85.

FIGURE VI

FIGURE VII

Fig. 6.

FIGURE VIII

To maintain working efficiency in his houses of industry, illumi-
nation needed to be greatly improved. First Thompson invented a
shadow photometer illustrated in Fig. VIII. Two lights to be com-
pared in intensity throw the shadows of the two little pillars on the
vertical field of the photometer, and the distances of the lights
adjusted to make the two shadows equally black. Since the in-
tensity of illumination from the light source varies inversely with
the square of the distance from the source, emitted intensities can
be accurately compared. He defined a standard candle for use
with his photometer which is almost identical to our present-day
standard " candle."

Having designed instruments to measure light, Sir Benjamin
turned his attention to improving lamps themselves. Lamps of
that day had the oil reservoir directly below the burner, and the
common whale oils were so viscous that as the lamp burned it
became more and more difficult for the oil to rise in the wick. Thus

the light became steadily weaker. Rumford designed one lamp which maintained the oil height on the wick by feeding the burner from a circular reservoir in the form of a hollow flat ring which surrounded the burner and was level with it. He invented another which accomplished the same purpose by having the oil reservoir on a side arm at the height of the burner. Both of these types are illustrated in Fig. IX.

In order to clothe the soldiers of the Bavarian army properly and economically, Thompson studied the transmission of heat through and radiation from all kinds of material of which clothes and blankets were made. This led him into a study of practical insulation, experiments which received wide attention and had great influence on the thinking of the engineers of his time.

In recognition of all his great endeavors on behalf of Bavaria, the Elector raised Thompson in 1791 to a Count of the Holy Roman Empire, and for this Thompson chose the title of Rumford in honor of the former name of the New England town where his fortunes had changed so spectacularly. With fame came also fortune, and his title is perpetuated by two large gifts of money to the Royal Society of London and the American Academy of Arts and Sciences for endowing the Rumford medals and premiums.

Count Rumford's ruthless and tactless introduction of his improvements in the face of the established order in Bavaria won him bitter enemies on all sides, and by 1799 it was evident to the Elector that he would have to get rid of his able Count. To do this with honor, he appointed Rumford Bavarian Minister to the English Court and Rumford left for London. However, this appointment was so unacceptable to George III (ostensibly because Rumford was a British subject, but really because of his previous behavior) that the Count could not even get an appointment to talk the matter over with the King's ministers. The Count thus found himself in London without a job, but he quickly busied himself with several projects. He applied through his friend Rufus King, American Ambassador to the Court of St. James, for the position of Superintendent of the U. S. Military Academy. This was looked on with great favor until investigation showed his past intelligence activities. Matters were then carefully arranged so that he would be offered the position publicly on the private assurance that he would refuse, which he did.
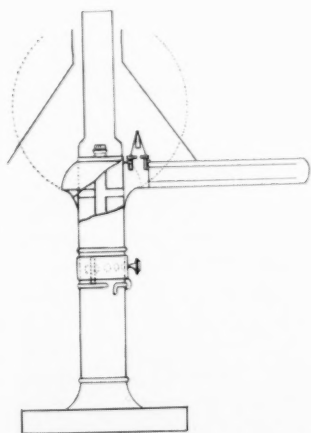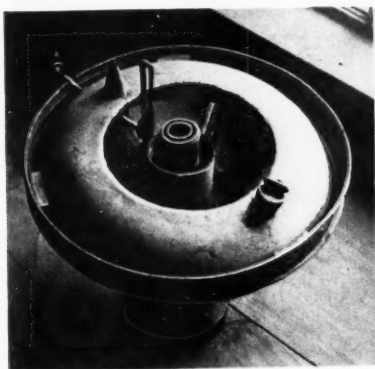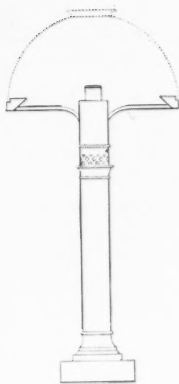
FIGURE IX

As many before him had done, he turned his attention in England to the perennial problem of smoky chimneys. Chimneys of his day were little more than large holes or flues connecting a fireplace to the roof above. In a series of brilliant deductions, Rumford reasoned that chimneys smoked because there were no clearly defined paths for the rising and descending air currents. The smoke shelf and throat which he introduced separated the warm from the cool air and set up orderly convection currents (currents which he himself had discovered some years before). "Before and after" drawings of his improvements are shown in Fig. X. So completely did Rumford solve the problem that essentially no improvements have been made in chimney construction since.

The occupation into which Count Rumford threw himself wholeheartedly around 1800 was the founding of the still famous Royal Institution of Great Britain. This was a scientific institution which, much like our Museums of Science today, was established to bring before the public the scientific and technological advances of the times. The Prospectus of the Royal Institution outlines its function and operation in the greatest detail. In listing those specific subjects which should be followed with the utmost vigor, one finds a complete listing of those facts of science and technology which interested Rumford to the total exclusion of those that did not. In building the lecture hall for the new institution, the Count designed the first successful steam-heating system and installed it as a working model for all to see.

Count Rumford's insistence that every detail of the operation of the Royal Institution be carried out exactly as he desired it brought about an intolerable situation within a very few years. The struggle between Rumford and others who were also responsible for the growth and operation of the institution resulted in a showdown for which Rumford never forgave his associates. He resigned in great anger, vowing that he would leave England never to return, a threat which he carried out.

He left London for Paris, where on previous visits he had been royally treated by French society and had found particular favor in the company of the widow of the famous French chemist Lavoisier. Not long after he settled in Paris he and Mme. Lavoisier married and his position in Parisian society appeared assured. However, Rumford's personality had never been an easy one to get
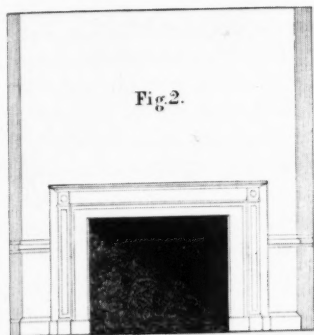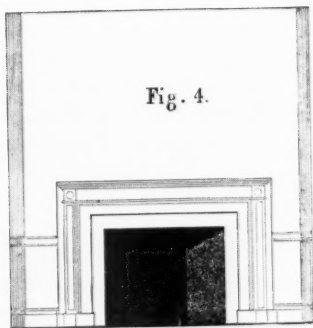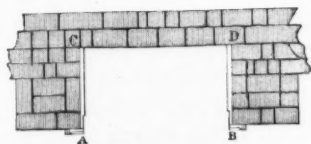
Fig. 2.

Fig. 4.

C   D

A   B

Fig. 1.

Fig. 3.

Scale of   1   2   3   4   5   Feet.
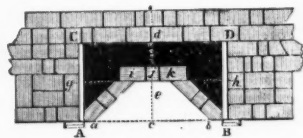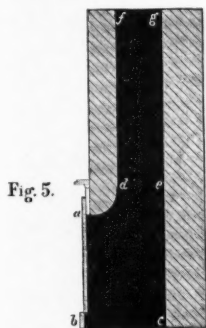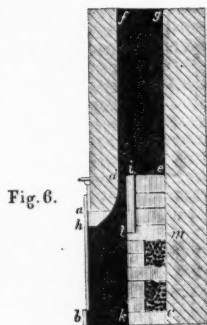
Scale of   1   2   3   4   5   Feet.

Fig. 5.

Fig. 6.

FIGURE X

along with, and age seemed to accentuate his eccentricities. He became embroiled in lawsuits over his improvements in lamp design, used his seat in the French Academy to oppose bitterly the scientific findings of numbers of French physicists and chemists, and fought publicly with his wife, who refused to abandon the name of Lavoisier in favor of Rumford. The story was told of how on one occasion a large party of guests rolled up to the gate of the estate, bidden by Madame to a soiree. Rumford saw them approach, rushed out, and ordered the gatekeeper to slam the gates in their faces. Then with a flourish he produced a key, locked the gate, and hurled the key into the river. Mme. Lavoisier retaliated by ordering the cook to prepare great vats of boiling salt water, which she poured over Rumford's prize rose gardens. After the formal separation took place, Rumford retired from the public view to the Parisian suburb of Auteuil, where he lived almost as a recluse, turning his whole attention to scientific pursuits.

In his last years at Auteuil, Rumford produced a tremendous number of papers on a very wide range of subjects. The following is only a partial list of titles of the period: " New Experiments in the Propagation of Heat in Fluids," " Use of Steam Heat in Distilling Brandy," "Adhesion of Water Molecules," " New Boiler to Save Fuel," " Steam Heat in Making Soap," " Spontaneous Mixture of Liquid " (a paper in which he first demonstrated the inter-diffusion of two liquids of different density), " Cooling of Liquids in Gilded and Ungilded Vessels," "Advantages of Wheels with Wide Felloes," " New Lamp," " Experiments with Wood and Charcoal," " Light from Combustion of Inflammable Bodies " (in which he demonstrated that the light from a candle came from incandescent solids), " Heat Developed in Combustion," " Heat of Condensation of Alcohol," " Heat Capacity of Liquids," and " The Structure of Wood."

The most fitting way to close this sketch of Count Rumford is to quote from a fellow scientist who knew the Count most intimately, and who had his confidence for many years. In pronouncing Rumford's eulogy before the French Academy, Baron Cuvier said of him:

" Nothing would have been wanting to his happiness, had the amenity of his behavior equalled his ardor for public utility. But it must be acknowledged that he manifested in his conversation

and in his whole conduct, a feeling which must appear very extraordinary in a man so uniformly well treated by others, and who had himself done so much good. It was without loving or esteeming his fellow creatures that he had done them all these services. Apparently the vile passions which he had observed in the wretches committed to his care, or those other passions which his good fortune had excited among his rivals, had soured him against human nature. Nor did he think that the care of their own welfare ought to be confided to men in common. That desire which seems to them so natural, of examining how they are ruled, was in his eyes but a factitious product of false knowledge. He considered the Chinese government as the nearest to perfection, because, in delivering up the people to the absolute power of men of knowledge alone, and in raising each of these in the hierarchy, according to the degree of his knowledge, it made in some measure so many millions of hands the passive organs of the will of a few good heads.

"An empire, such as he conceived, would not have been more difficult for him to manage, than his barracks and poor-houses. For this he trusted especially to the power of order. He called order the necessary auxiliary of genius, the only possible instrument of real good, and almost a subordinate divinity regulating this lower world. He himself, in his person was, in all imaginable points, a model of order. His wants, his labors, and his pleasures were calculated like his experiments. In short, he permitted in himself nothing superfluous, not even a step or a word, and it was in the strictest sense that he took the word *superfluous*.

" This was no doubt a sure means of devoting his whole strength to useful pursuits, but it could not make him an agreeable being in the society of his fellows. The world requires a little more freedom, and is so constituted that a certain height of perfection often appears to it a defect, when the person does not take as much pains to conceal his knowledge as he has taken to acquire it.

" This rigorous observance of order, which probably marred the pleasures of his life, did not contribute to prolong it. A sudden and violent fever carried him off at the age of 61. He died on the 12th August 1814 in his country house at Auteuil."

The Scientific Work of Enrico Fermi

PETER DEBYE
Cornell University

This statement concerning the work of Dr. Fermi was made on the occasion of his being awarded the Rumford gold and silver medals for his studies of radiation theory and nuclear energy by the American Academy of Arts and Sciences in Boston on March 27, 1953.

Fifty-one and a half years ago, on the 29th of September, 1901, Enrico Fermi was born in Rome. He studied at the University of Pisa. After completing his studies in 1922, he went to Göttingen, worked with M. Born, and traveled as far north as Leiden. In 1924 he was appointed a lecturer at the University of Florence.

As we look at his work during these first years of his career we meet with publications handling problems often discussed at that time. Some physicists were troubled by the fact that apparently a nasty infinity crops up when the thermal equilibrium of a hydrogen nucleus and its electron is considered. Fermi shows that there is no trouble when the actual case of many hydrogen atoms at finite distances from each other is considered.

Another problem, treated previously by N. Bohr, concerned electrons or a-particles traveling through a gas. Fermi handled the problem in developing the electrical field of the moving charge at the position of the gas atom or molecule in a Fourier time-series, and therefore, in essence, handled the case as if the atom or molecule were exposed to light waves of all frequencies, from zero to infinity. Characteristic for the state of quantum-theory at that time is the way in which Fermi helps himself: he cuts off the series at a frequency determined, by means of Planck's quantum, by the kinetic energy of the flying particle.

With Rasetti he considered and treated the effect of a magnetic field on resonance-radiation in the interesting case in which the field is not constant but periodic in time.

In 1926 Fermi published a paper which was of great importance for the further development — a real hit. It is entitled (in trans-

lation), "Concerning the Quantization of the Ideal Monatomic Gas." In 1925, inspired by a paper published by Stoner a year before, Pauli had formulated the "Pauli principle," according to which one quantum state cannot accommodate more than one electron. Fermi observed that the classical theory of the Equation of State of ideal gases does not agree with the Nernst theorem, which requires the entropy of all substances in thermal equilibrium to be essentially equal, say zero, at the absolute zero of temperature. He saw that if the Pauli principle is applied to gas atoms, this discrepancy can be avoided and fundamentally important deviations of the classical laws of ideal gas behavior have to appear at sufficiently low temperatures. The reasoning employed by Fermi is of course based on Boltzmann's relation between entropy and probability. However, the way in which the counting is performed in order to find this so-called probability is and has to be totally different from the counting which Boltzmann used.

I could not but feel, when for the purpose of this introduction I read the paper again, that Fermi, when he wrote it, must have been disturbed a good deal about the fact that for ideal gases his calculation gives deviations from the classical laws of opposite sign to those calculated a short time before by Einstein for the same case. This discrepancy only shows how much we still had to learn in 1926. We now know that both Fermi and Einstein were right; we have Bose-Einstein and Fermi-Dirac statistics, and which one to use depends on the parity of the objects. For electrons, Fermi's theory applies and it is hard to imagine what we could do nowadays about the understanding of, say, conductivity in metals and semi-conductors without knowing about the Fermi-level.

In 1927 Fermi became Professor of Theoretical Physics at the University in Rome. He married in 1928 and stayed in Rome until shortly after receiving the Nobel Award, when he transferred to Columbia University in 1939 and to the Institute of Nuclear Physics in Chicago in 1945.

During his first years in Rome Fermi was still mostly interested in atomic and molecular structure — in the outside of the nucleus. He wrote about the analogy in quantum theory of the impact of a mass-point on a rotator, and on the optical theory of a grating. He showed how from the observation of the anomalous dispersion in the vicinity of two thallium lines the quotient of Planck's constant

h to Boltzmann's constant k can be determined. He demonstrated how dependent the calculated intensity quotient of the alkali doublet is on the choice of the eigenfunctions used in the calculation, and explained in this way the observed quotients different from the ratio 2:1. He showed how an observed double, instead of an expected single line in the Raman spectrum of the linear molecule of CO_2, can be traced back to an accident concerning the numerical value of the quotient between the frequencies of two possible modes of vibration of the molecule. With Rasetti he demonstrated by a very ingenious experimental method the Raman effect of rock salt, which, according to a first-approximation theory, should not exist at all.

However, in 1933 the atomic nucleus began to appear in Fermi's thinking. First he is still outside when with Segré he considers the effect of magnetic nuclear moments on the hyperfine structure of spectral lines. One year later he is already deep inside when he publishes his theory of β-decay. Immediately following comes his famous series of publications that tell (1) of artificial radioactivity induced by neutron bombardment; (2) of how neutrons can be slowed down by giving them an opportunity to collide with H nuclei in passing them through materials composed of molecules that contain much hydrogen; (3) of the possible production of elements with atomic numbers exceeding 92.

The ensuing story of the enormous development which occurred during Fermi's time at Columbia University and which led to neutron chain-reactions and the construction of the so-called " atomic " bomb is too well known and too fresh in our minds to be dwelt upon at this time; it can only be reverently mentioned.

This evening Fermi receives a medal founded by a man with a great scientific mind, who at the same time was an international character. It seems symbolic that you are witnessing here in the heart of Boston the spectacle of one Holland-born introducing one Italian-born to receive a reward brought into existence by the munificent gesture of one American-born, who was a Bavarian Count. It is presented to the recipient for his accomplishments in the field of light and heat. Fermi made possible the highest concentration of light and heat in one point ever man-made. In this Academy we value no less his scientific deeds. Thinking about physicists who are trying to distill quantitative theories and pre-

dictions from the observed facts, it soon becomes apparent that we have to make a distinction between mathematical and theoretical physicists. The first rides the wild horses of mathematics as at a rodeo and believes in an active predestination that will take care to see him deposited in the right spot. Occasionally it even works out all right. The other — he rides the horses too, but he knows how to tame them and force them to go his way. This is how I see Fermi doing it with consummate skill.

The Scientific Work of Willis E. Lamb

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This statement concerning the work of Dr. Lamb was made on the occasion of his being awarded the Rumford gold and silver medals for his studies of the atomic hydrogen spectrum by the American Academy of Arts and Sciences in Boston on March 27, 1953.

Professor Lamb received his doctoral degree from the University of California in Berkeley in 1938 for his theoretical research under the supervision of J. Robert Oppenheimer. From Berkeley, he went to Columbia University where he remained for the next twelve years. From Columbia, Lamb went to Stanford University where he now resides. In the few years that elapsed between his Ph.D. degree and the beginning of the war, he wrote various theoretical papers and did a superb job of teaching at Columbia.

Of Lamb's papers written during the pre-war period, I should like chiefly to speak of two. These have the rather intriguing characteristic that at the time they were written they appeared to be relatively unimportant and of limited application to a single small field. Only in the recent post-war years have physicists come to realize that each of these papers was fundamental in what has grown to be a large and important field.

One of these pre-war papers was Lamb's discussion of the magnetic shielding of nuclei in atoms. When the magnetic moment of a nucleus is measured by its interaction with an external magnetic field, an important correction must be made for the fact that the diamagnetic circulation of the atomic electrons also produces a magnetic field at the nucleus. Therefore, the nucleus precesses not alone in the known external magnetic field but in the resultant of this field and the unknown local field due to this induced electron circulation. Professor Lamb's paper was the first one which provided a means for calculating the correction for the induced magnetic field. At the time of this paper it appeared that Lamb's theory was of rather limited application to a few molecular beam experi-

ments. However, in recent years new techniques of nuclear
moment measurements have greatly expanded the field. In ad-
dition, Lamb's fundamental paper on magnetic shielding has been
extended to polyatomic molecules. The application of these
theories to measurements with such molecules have in the past few
years developed into a very fruitful means for studying molecular
structure.

A similar history has accompanied the early paper of Lamb and
of B. T. Feld on nuclear quadrupole moment interactions in mole-
cules. Originally this paper was thought to have applications to
only a few molecules which enter into molecular beam experiments.
Subsequently, however, the paper of Lamb and Feld has been the
foundation for many theoretical papers interpreting both nuclear
resonance experiments and the extensive experiments of micro-
wave spectroscopy.

During the last war, Professor Lamb developed both theories
and magnetrons in the Columbia Radiation Laboratory. Indeed
this work was his real start in experimental physics. There was
only one rule in this laboratory and that was that everyone had
to make magnetrons; so Lamb — though a theorist — made his, and
they were good ones, too. Those of you who heard Lamb's talk
today* could see from his slides how he profited from his Radiation
Laboratory experience, since such apparatus could have been de-
signed only by a man experienced in the techniques of sealed-off
vacuum tubes.

After the war, Lamb began at Columbia his most important
experiment and, indeed, one of the most significant of all post-war
experiments. He measured with high precision the energy separa-
tion of the different levels of atomic hydrogen in its second ex-
cited state. Although the separations of these levels had been
studied optically by Houston and others, the difficulty of the
optical experiments was so great that contradictory results were
obtained by different observers. Lamb, however, measured di-
rectly the separations by allowing microwave radiations to be
emitted and absorbed. In this way he could make his measure-
ments with high precision and found a marked disagreement with
the Dirac electron theory. In particular, for two levels whose

*Summarized in a later chapter.

frequency separation should have been exactly o megacycles by Dirac's theory, Lamb found the far different result of 1057.77±0.10 megacycles, which is frequently spoken of as the Lamb shift.

Once this value was found experimentally, theorists were able by a process of " postdiction " (as opposed to prediction) to re-examine their theories and say that they should have suspected such a result all along. However, this very process of re-examination has led to one of the major theoretical developments of recent years — namely, the development of suitable procedures for making quantum electrodynamic calculations. According to these " postdictions," the source of the Lamb shift was the interaction of the electron with both the electromagnetic field and the field of negative energy electrons which affect an electron even when the fields are in their lowest state. The precise calculation of the Lamb shift is a very complicated one indeed and occupied theoretical physicists for several years. It is significant to note that the first "postdicter " to reach and publish the correct answer was also Willis Lamb, functioning this time as a theorist instead of as an experimentalist.

Another important measurement to come from the same experiment was the precision value for the fine structure constant, $a = e^2/\hbar c$, one of the intriguing dimensionless numbers which so frequently occurs in quantum electrodynamics. His result was $1/a = 1/(137.0365 \pm 0.0012)$.

The only debatable point about the award's going to Professor Lamb was the question of whether or not his research was really in the field of light and heat. For three different reasons the committee felt it was. One reason is that microwaves are merely an extension of the same electromagnetic radiation as ordinary light and are merely in a frequency interval unknown in Count Rumford's day. A second reason is that the same energy levels studied by Lamb can be studied by means of optical spectroscopy. A measurement of these same quantities with a hundredth of Lamb's accuracy by optical means would have been a perfect basis for the Rumford award, and it seemed inappropriate to withhold the award from Lamb merely because of his much greater accuracy. Finally, it should be noted that in 1926 the premium was given to Professor Arthur Compton for his X-ray studies. His wavelengths

are shorter than the optical ones by as much as Lamb's are longer. In fact, it is of interest to observe that the geometric mean between the wavelengths characteristic of X rays and those of microwaves is just within the visible spectrum. So even if Lamb's experiments were not regarded as strictly within the fields of light and heat, the award would be justified as a righting of the past wrong of Professor Compton's award.

The Scientific Work of Lars Onsager

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This statement concerning the work of Dr. Onsager was made on the occasion of his being awarded the Rumford gold and silver medals for his contributions to the thermodynamics of transport processes by the American Academy of Arts and Sciences in Boston on March 27, 1953.

Lars Onsager began the study of science at the Norwegian Technical Institute in Trondheim, Norway, from which he received the degree of Chemical Engineer in 1925. He then went to Zürich, where he worked under the direction of Peter Debye during the period 1926-1928. He came to the United States in 1928 as Associate in Chemistry at the Johns Hopkins University. Shortly thereafter he went to Brown University as Research Instructor, where he remained until 1933. In 1934 he became Assistant Professor of Chemistry in Yale University, and since 1945 has held the Willard Gibbs Professorship of Theoretical Chemistry at Yale. He received his Ph.D. from Yale University in 1935.

The early papers of the Debye-Hückel theory of electrolyte solutions appeared during Onsager's period of study at Trondheim. They caught his attention and led to his early interest in the theory of electrolyte solutions, to which he has since made so many important contributions. Working independently in Norway, Onsager achieved certain important and necessary improvements in the theory of electrolytic conductivity, which had already received preliminary treatment by Debye. He completed this work in Zürich and published the results in 1926 and 1927. His interest in electrolytes continued, and in 1932 he published with Fuoss a definitive paper on irreversible processes in electrolyte solutions, in which a complete and general theory of electrolytic conductivity and diffusion in multi-valent electrolyte mixtures was presented. In 1933, Onsager published a general critique of the Debye-Hückel theory, based on the principles of statistical mechanics. By investigating the conditions of integrability of the average

electrostatic potential in the neighborhood of an ion, he was able to meet certain objections which had been raised against the theory by R. H. Fowler and to show that the linearized form of the Debye-Hückel theory was consistent with statistical mechanics. In this same field Onsager achieved a theoretical treatment of the Wien effect in weak electrolytes and, in collaboration with Samaras, a rigorous treatment of the surface tension of electrolyte solutions.

While still a student in Norway, Onsager began to give thought to the general theory of irreversible processes. He continued to develop his ideas on this subject during his stay in Zürich and during his early years in the United States. In 1931 he published two important papers on irreversible processes, in which he formulated his reciprocal relations, which form the basis of irreversible thermodynamics. For small departures from thermodynamic equilibrium, the currents of heat and matter may be expressed as linear functions of the gradients of temperature and of the chemical potentials of the several components of a system. Onsager formulated the general principle, earlier foreshadowed by Thompson in his treatment of thermoelectric potentials, that the matrix of the coefficients relating the currents to the gradients of the potentials is symmetric. This principle may be regarded as a phenomenological law, with the same status as the first and second laws of thermodynamics, to be verified directly by experiment. As Onsager demonstrated, it may also be regarded as a consequence of a more general principle, the principle of least dissipation, which states that the rate of increase of entropy arising from a set of coupled irreversible processes is a minimum. The principle may be justified from the molecular point of view on the basis of considerations arising from the principle of microscopic reversibility.

In 1936 Onsager published an important paper on the dielectric polarization of polar liquids, in which he demonstrated that a drastic modification in the Lorentz internal field was necessary in polar dielectrics. His simple and penetrating analysis of this problem swept away puzzling paradoxes, arising from the incorrect use of the Lorentz field, and clarified the relationship between the dielectric constants of polar liquids and their structure on the molecular scale. Among the important consequences of this theory was the correct interpretation of the dielectric constant increments pro-

duced in aqueous solution by the amino acids and proteins, which possess large permanent electric dipole moments.

In 1940, Kramers and Wannier demonstrated that the partition function of a crystal consisting of N atoms could be expressed as the Nth power of the largest eigenvalue of a matrix operating on a vector in a space having a number of dimensions equal to the number of states accessible to each atom. They applied their method to the calculation of the partition function of a linear chain of atoms in the problem of classical ferromagnetism. However, extension of their method to two- and three-dimensional arrays of atoms involves extraordinary and formidable mathematical difficulties. In a paper in 1944 on crystal statistics, Onsager, accomplishing a feat of remarkable mathematical virtuosity, was able to express the partition function for a two-dimensional array of classical ferromagnets in the closed form of a definite integral. In a later paper in 1947, written in collaboration with Miss Kaufmann, the mathematical treatment of the problem was materially simplified with the aid of the theory of spinors.

In addition to Onsager's contributions to statistical mechanics and thermodynamics, which we have chosen to discuss here, he has made a number of other important contributions, among them in 1948 a theory of long-range orientational order in solutions of tobacco mosaic virus, in 1939 a contribution to the theory of thermal diffusion, and in 1952 an explanation of the Haas-Van Alphen effect. He is currently engaged in work on problems relating to Liquid Helium II, the properties of which are a macroscopic manifestation of quantum mechanics.

Onsager's scientific work is characterized not only by mathematical virtuosity but also by profound physical insight and intuition. His accomplishments are impressive and of lasting value. It is therefore most appropriate that he has been selected for a Rumford Premium Award by the American Academy of Arts and Sciences on the occasion of the 200th anniversary of Count Rumford's birth.

Reflections on Thermodynamics [*]

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The two laws of thermodynamics are, I suppose, accepted by physicists as perhaps the most secure generalization from experience that we have. The physicist does not hesitate to apply the two laws to any concrete physical situation in the confidence that nature will not let him down. But although he applies the laws with sureness, it does not necessarily follow that he could justify with equal confidence the soundness of his formulation of the laws or reproduce the argument on which the laws rest. The average physicist does not feel the need to give much attention to this sort of thing, and it would therefore not be surprising if the logical foundation on which thermodynamics rests is less satisfactory than the superstructure. This sort of situation is not unique for thermodynamics or for physicists. Most mathematicians, for example, in their moments of candor, will admit that they are much less confident of the logical foundations of mathematics than they are of its body of practise. The historical development of thermodynamics has been such, however, that it has been particularly susceptible to logical insecurity, since the laws were formulated in their present form by the great founders of thermodynamics, Kelvin and Clausius, before all the essential physical facts were in, and there has been no adequate reexamination of the fundamentals since.

The purpose of this talk is to direct attention to some of the more unsatisfactory logical features of our present thermodynamics. It seems to me that there are two main areas in which the logical situation is still unsatisfactory. There is in the first place the area of phenomena concerned with the first law and with heat as a form of motion. These phenomena were pretty well explored before the formulation of the first law, but nevertheless I believe that there were logical implications which are evident only now with our present background of quantum mechanics. In the second place, there are the fluctuation phenomena typified by the Brownian motion, not known to the

founders of thermodynamics. It seems to me that had these phenomena been known they would have left some explicit mark in the formulation of the laws.

We consider now the phenomena of the first area, the area of conservation of energy and of heat as a mode of motion. There was a long historical background in long and obstinately unsuccessful attempts to produce a perpetual motion machine. The early devices were almost entirely of a mechanical nature, and go back to the time when the laws of static equilibrium of forces were not properly appreciated. With the formulation and study of these laws it became apparent that no purely mechanical device could ever do more than keep running forever, and that if it did run forever no useful work could be got out of it. The reason that actual machines did not run forever is that they are imperfect, and this imperfection took the form of friction or inelastic collisions. When knowledge of mechanics had progressed far enough for kinetic and potential energy to be recognized, it was seen that mechanical systems conserve their energy, except for the action of the so-called imperfections, friction and inelasticity. Whereupon a conceptual invention was made, the perfect mechanical system, with no friction or other source of loss. These systems could be approached in practise, so that in practise mechanical conservation could be approached in certain types of system, but actually all mechanical systems lost energy.

The next great experimental step was the discovery that inelastic collisions and the overcoming of friction are always accompanied by the appearance of heat. It was not long before quantitative relations were being found here, a development in which our Count Rumford played such an important part, leading presently to the accurate determination of the mechanical equivalent of heat by Joule. That is, the disappearance of a definite amount of mechanical work was found to be always accompanied by the appearance of a definite amount of heat. The converse process, the disappearance of heat and the appearance of mechanical energy, was also a process which was found to be subject to mathematical regularities. The full convertibility of heat into work was, however, subject to restrictions not applying to the direct process. These restrictions were formulated in the second law.

With the discovery of the interconvertibility of heat and mechanical work (I use " mechanical " in a general sense to include electrical, magnetic, etc.), the physicist should logically have stopped. That is, we recognize energy in two forms, thermal energy and generalized mechanical energy, and it is the two forms together which are conserved. The physicist, however, would not stop here, but as a human being had to have his explanation, which he achieved in a flash of insight by postulating that heat energy is mechanical energy of the small parts of which matter is composed. The conservation of energy thus becomes nothing but the conservation of mechanical energy, mechanical energy of the ultimate particles. However, these ultimate particles behave like no known mechanical system, but only like systems which are sometimes approached in practise when we can minimize the imperfections of friction and inelasticity. That is, a microscopic mechanical system is postulated, like no known actual mechanical system, but highly idealized. Why was the physicist so pleased with his idealization and so sure that he had the right answer? What was the experimental evidence that microscopic mechanical systems do not have the imperfections of macroscopic mechanical systems? There was, of course, no experimental evidence. The picture was so appealing because it was consistent with a long-standing picture of matter as consisting of ultimate hard massive particles with no internal structure and completely described by their mass, position, and velocity. The argument for the analysis of matter into such particles is a purely metaphysical argument, somehow deeply rooted and congenial to human habits of thought, and going back to at least the time of Lucretius.

The assumption of particles resolves as it were by fiat a dilemma which the physicists of the time were not sophisticated enough to see, but which later did bedevil physics for years. For if matter is not composed ultimately of discrete particles but is truly continuous, and if the laws of mechanics apply to such continuous matter, then why do not all macroscopic motions dissipate themselves by burrowing deeper and deeper until they finally disappear? This is the same catastrophe that was later known as the ultraviolet catastrophe in connection with the luminiferous ether, which was assumed to be truly continuous. The ultraviolet

catastrophe was avoided by postulating the discrete character of radiation demanded by quantum theory. Logically the postulation of massive particles with no internal degrees of freedom would seem to be much akin to quantum theory, but I believe that we cannot, for that reason, absolve either Lucretius or Rumford from having been pretty metaphysical.

There are other respects in which the classical formulation of the first law is at loose ends logically. It is recognized that energy can be imparted to a body in two forms, as mechanical energy in the form of mechanical work, and as thermal energy in the form of heat. Now although it is assumed that the distinction between heat and work is possible, no directions are usually given as to how the distinction is to be made in the laboratory, but the matter is left to the instinct of the experimenter. It proves that usually the instinct of the experimenter is safe enough, but nevertheless it is not difficult to embarrass him if one makes the effort. In order to escape embarrassment the experimenter usually arranges matters so that he deals only with heat without mechanical work or with mechanical work without heat, for it is only when the two occur together that difficulties arise. But it is not always possible to separate heat and work with the instruments of the laboratory, and we are forced to deal with the situation conceptually. This we do by saying that we still have conservation in such cases, and that although we can no longer distinguish a stream of thermal energy and a stream of mechanical energy entering the body, the two streams merge into a stream of generalized energy. No instrument responds to this stream of generalized energy, the meaning of which must be sought entirely at the paper and pencil level in the doings of the ultimate particles and in the microscopic electromagnetic field. The necessity of recognizing a stream of generalized energy does not usually embarrass us, because in practise we can usually find methods of changing the state of the body under circumstances such that work and heat are defined, and we can then play the game backward and calculate mathematically what the stream of generalized energy must have been, in terms of its divergence, which is given by experiment. But if the body undergoes changes which intrinsically can never be analyzed into heat and work, and we have no way of knowing at present that there may not be such situations, then

it is not known whether conservation is true or even whether energy has meaning.

There is one comparatively minor matter in the classical analysis of energy into heat and work which I think has never been satisfactorily formulated. It is usually assumed that the work and heat received by systems may be evaluated by determining the work and the heat given to them by the external universe. This treatment fails in the case of friction, because the external universe delivers mechanical work which the system receives as heat. The difficulty would seem to be connected with the surface of discontinuity on which friction takes place. The difficulty here is only formal, although it is seldom recognized, and I believe can be surmounted by a special formulation when systems of discontinuity occur.

We now consider the second area of thermodynamic phenomena, the area of fluctuation phenomena, which were not known when the laws of thermodynamics were formulated. The logical dilemma thus thrust upon us is obvious, because it appears that the fundamental physical assumption used in defining temperature does not correspond to the facts. Temperature is defined in terms of equilibrium states, but if we make our instruments delicate enough we find that equilibrium never occurs. The smooth, continuously variable states assumed in classical thermodynamics are only an approximation, which in the early days appeared to be an approximation that we could approach as closely as we pleased, but which we now see recedes from us as we make our instruments as fine as we have now found how to do. The situation is like that presented by a semi-convergent series in mathematics. The universe of thermodynamics is the universe of the flat plateau of apparent convergence.

With the appearance of fluctuation phenomena all sorts of questions arise. Can we formulate the restrictions to which our operations must be subject if we want to be sure of remaining on the plateau of thermodynamics? The original expectation was that the universe of operations of thermodynamics included all the operations that we were capable of performing physically. Now we see that there must be restrictions, but what are they? How small is it permissible to make our instruments if the results we find are to satisfy thermodynamics? or how long must we

wait for equilibrium before thermodynamics will give us an approximate answer? Practical questions arise. Is it possible by ingenious enough construction to violate the second law of thermodynamics on an industrially profitable scale? The answer is not obvious, and the invention of his demon by Maxwell has raised genuine doubts in many minds. The answer is not to be found in the completely random character of the small-scale fluctuations, for it is possible on a larger scale to extract useful energy from the completely random motion of the waves of the sea, for example. This can be done by the use of a ratchet mechanism. We have to ask why a ratchet mechanism is impossible on a scale small enough to deal with fluctuations. It is no answer to say that the ratchet would be subject to fluctuations, and so could not function, until we have shown how the different sorts of phenomena, such as the optical and the tactual, leave the plateau together as we progress toward the microscopic. But to show this would be pretty nearly equivalent to assuming what we would like to prove.

The average physicist reacts to these considerations by admitting that thermodynamics is a tool of limited applicability, which must be replaced by the more powerful tool of statistical mechanics if one wants really to understand microscopic phenomena. At first it was the statistical mechanics based on classical mechanics, but now this has to be supplemented by quantum mechanics, in which the notion of probability is brought in at the beginning, as an intuitive concept, not to be explained. I believe there can be no question but that statistical mechanics is a satisfactory and powerful tool in providing answers to concrete problems; our concern here is rather with its logical status. Consider first classical statistical mechanics. The fundamental laws here are those of Newtonian mechanics applied to a system of particles. But a hard particle, with only mass, position, and velocity, is a construction for which no logical justification whatever can be given. In assuming that energy does not reside inside the elementary units we have by fiat sidestepped the whole logical difficulty. Because of this, classical statistical mechanics in no wise has the character of an explanation in terms of which we can understand, but rather it must be regarded as a discovery, which enables us to compute correct answers, but which does

not afford the understanding for which we had hoped. One of the recognized difficulties of classical statistical mechanics is that logical considerations of probability are inapplicable to concrete situations. It has seemed to many that this difficulty is turned by quantum mechanics by introducing probability concepts into the very foundations on an intuitive basis, and by recognizing that the results are not applicable to single concrete instances, but are applicable only to ensembles. I believe, however, that the fundamental logical difficulty is not to be removed in this way, because logically actual ensembles are concrete systems, of a higher order it is true, but nevertheless concrete, and therefore incapable of admitting such operations as, for example, taking mathematical limits as is necessary in making the concrete application. Both classical and quantum statistical mechanics must be recognized to be paper and pencil devices, of which it is meaningless to ask whether the description they afford of microscopic phenomena is true or not. In general, the meaning of our concepts on the microscopic level is ultimately to be sought in operations on the macroscopic level. The reason is simply that we, for whom the meanings exist, operate on the macroscopic level. The reduction of the meanings of quantum mechanics to the macroscopic level has, I believe, not yet been successfully accomplished and is one of the major tasks ahead of quantum theory.

In the meantime the spirit of the approach of thermodynamics is from this point of view logically satisfying, because the operations of thermodynamics are macroscopic operations. We are driven, I think, by the existence of fluctuation phenomena to recognize that we can never expect, as we had at one time hoped, for a logically clean-cut scheme of unlimited physical applicability on the macroscopic level. But this, I believe, is no more than we are driven to from every other line of physical analysis, when we push the analysis as far as we can. Sharpness and freedom from fuzziness have never been attained, not even in the operations of logic itself. In the meantime, thermodynamics, with its use of large-scale operations, stands as a sort of prototype of the best that we shall ever be able to do.

In the last few years it has been appearing that we can push further with the macroscopic analysis than at one time seemed possible. It has always been a reproach to thermodynamics that

its handling of irreversible phenomena was inadequate or even impotent. We are now finding how to deal with large and important groups of irreversible phenomena by methods in the spirit of thermodynamics. For there are groups of irreversible phenomena which are adequately describable in terms of macroscopic operations, such as heat flow by conduction, or resistance heating by an electrical current, or the motion of a viscous liquid. These phenomena can now be handled by a broadening of the classical concepts of thermodynamics. These broadened concepts include the recognition of fluxes of heat and mechanical energy and generalized energy, as well as fluxes of entropy and creation functions for entropy. It is possible, I believe, to go even further in the treatment of irreversible phenomena in the spirit of thermodynamics. Classical thermodynamics defines the entropy only of those states of a body which can be reached by some reversible process from a standard state. Such a definition rules out on principle most of the matter of daily life, because most states can be subject to no reversible displacement whatever — any plastically deformed metal is an example, or any biological system. It is possible to generalize the entropy concept for some simple systems subject to essential irreversibility of this sort, and to find the specific form of the entropy function in terms of the parameters of state by imposing the requirement that the total entropy of the universe must increase for every possible method of coupling the system with the external universe and for every possible displacement. I believe that in general the analysis of such systems will be furthered by the recognition of a new type of large-scale thermodynamic parameter of state, namely the parameter of state which can be measured but not controlled. Examples are the order-disorder rearrangements in mixed crystals, measurable by X-rays, and the dislocations in a solid, measurable by the attentuation of supersonic vibrations. These parameters are measurable, but they are not controllable, which means that they are coupled to no external force variable which might provide the means of control. And not being coupled to a force variable, they cannot take part in mechanical work. Such a parameter of state, which enters into no term in the mechanical work, can be shown by simple analysis to be one which can take part only in irreversible changes. In working out the thermo-

dynamics of actual systems in which such uncontrollable par-
ameters play a part, the first step will be the determination of
the equation of state in terms of the new parameters. Such will
be a matter of considerable difficulty, because the effects of such
parameters are usually obscure and small. However, this is an
essential step and until it is taken we cannot hope to master the
thermodynamics of such systems.

The Management of Flame

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1. *Introduction.* The title of this communication is suggested by the title of Rumford's Sixth Essay: " Of the Management of Fire and the Economy of Fuel." Rumford is famous for a great many achievements, but foremost of all these was his realisation of the importance to civilisation of the right management of fire and heat. Even today full heed has not been given to his doctrine. In recent years perhaps there has been a greater urge to utilise energy with less waste, but even so the waste is far greater than the energy usefully applied.

The Management of Fire and the Economy of Fuel is not the concern of this note. That would be too vast an issue in these days! " The Management of Flame " limits discussion to the burning of gases, but it is a wide subject with many aspects. There is, however, need to manage flame whether it be for the design and control of furnaces, or for internal combustion engines or turbojet engines, or rocket motors, or just for burner flames. Up to the present flame has not been very manageable, in the sense that the exact combustion behaviour could not be predicted, under given circumstances in a particular contrivance, in the way that aerodynamic behaviour can generally be assessed through application of model laws. Efforts have been made in recent years to study more closely the combustion of gaseous mixtures with a view eventually of making the combustion processes in practise more amenable to calculation. Besides the actual chemical oxidation of the fuel there are many other steps in the process which have to be considered, such as mixing of the gaseous fuel with air, and heat transfer by conduction and radiation. With *liquid fuels*, as well as mixing, there are complicated questions of droplet distribution, penetration, evaporation, transfer of heat to the droplet for vaporisation, and mass transfer of the vapour from the droplet. Similar problems arise in disperse systems of solid combustibles. The simple combustion process in the gaseous flame itself is quite enough

of a problem to manage and it is that with which my note will mainly be concerned.

2. *The Simple Flame.* It is customary to start by considering a plane flame front, which remains stationary because the unburnt gas mixture flows into the flame zone as fast as the flame consumes it; the hot mixture of burnt gases flows away.

The burning velocity is defined as the mass of gas mixture burnt per unit area of flame surface per second. When the flame is situated as a plane at right angles to the lines of flow of the gas mixture, then this definition suffices, and as $M = V\rho$, where ρ is the density and V the volume of the gas at normal temperature and pressure, V is more often specified in the figure given for the burning velocity.

This is for premixed flames. Many flames (in fact most met with in practise) are diffusion flames (or only partly diffusion and partly premixed). A diffusion flame, like a candle flame, is one in which the fuel and the supporters have to diffuse the one into the other and the combustion is mainly dependent, not on the burning velocity, but on the mutual rate of diffusion of the gases.

We shall be concerned mainly with the premixed flame.

Consider first the plane flame:
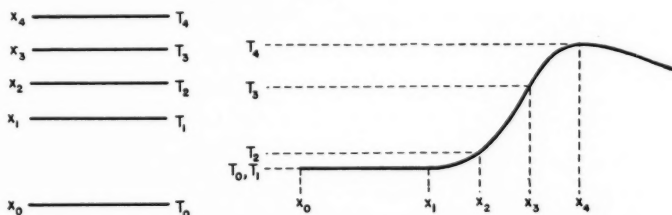
At x_0 and T_0, the gas is streaming in at the normal temperature and pressure;

At x_1 and T_1, owing to conduction of heat from the flame zone, the temperature is beginning to rise perceptibly;

At x_2 and T_2, the gas has started to react at an appreciable rate;

At x_3 and T_3, the reaction is so fast that luminous combustion is observable; and

At x_4 and T_4, the reaction is complete. Thereafter, the burnt gases start to cool.

In order to analyse a flame the change of the magnitude of one or more variables must be chosen to describe the behaviour. Temperature can be chosen, though burning velocity, density change, or extent of reaction in principle could be used. Although *inside* the flame, temperature equilibrium may not have time to be established in some positions, yet in each such position or plane there must be a temperature assignment. The following equation is a statement of all the possible variables influencing temperature for the one individual case of a plane flame front (the change in any of the variables being perpendicular to the plane flame front).

$$-\frac{\partial}{\partial x} K\left(\frac{\partial T}{\partial x}\right) - M \frac{\partial}{\partial x}\left(TC\right) + M \frac{\partial}{\partial x} \sum_n Q_n \, \epsilon_n + \frac{\partial R}{\partial x} = 0$$

K = Conductivity (varying with T and composition).
M = Mass burnt per unit area per unit time.
C = Specific heat at constant pressure.
Q_n = Heat of reaction, n = number of different reactions taking place.
  ϵ = Fraction of total reaction completed at position x.
R = Radiant flux.

Integration between x_0 and x $\left[\left(\frac{\partial T}{\partial x}\right)_{x=0} = 0 \text{ and } \epsilon_x = 0, \; x \text{ referring to unburnt gas}\right]$

gives: $K \frac{\partial T}{\partial x} + M \sum_n Q_n \, \epsilon_n = M\left(TC - T_0 C_0\right),$

for $(R-R_0)$ is a minor factor in the preignition zone and can be neglected.

Even in this simple form[1], the equation does not lead to an estimate of the burning velocity from a knowledge of the temperature in any position x_1, for K and C vary with temperature and with the amount of reaction. This latter quantity depends on knowledge of the rate of the reactions; it is only permissible to write Q_ϵ for $\sum_n Q_n \, \epsilon_n$ if each participating reaction through each layer is completed to a proportionate extent; no spatial separation of component reactions must occur. If, however, accurate means

of measuring the burning velocity and the temperature parameter
through the various flame zones can be found, the application of
methods of successive approximation can be applied to make the
equation yield values of Q_ϵ, the extent of reaction as measured by
the heat contributed. The enthalpy distribution in the flame can
so be found. Beyond the flame front there is flow of energy by
conduction back against the direction of average material flow.
This flow becomes greater, the greater the release of energy and the
higher the temperature of the particular region of the flame.
Eventually in spite of still rising temperature, the rate of supply
of energy diminishes, but the loss by conduction continues to
increase, so somewhere in the flame there is an enthalpy hump.
It is of considerable interest to determine the position of the hump,
for this will provide information as to conditions of temperature
equilibrium in the flame. An accurate knowledge of the tem-
perature distribution from layer to layer and of the extent of the
reaction can then be used to compare with figures derived from
the measured kinetics of the homogeneous reactions at temperatures
encountered in the flame zones, and so to discover whether the
heat transfer (by conduction) from the flame into the unburnt
gas or the mass transfer (by diffusion of radicals) from the flame
is of most consequence in maintaining the propagation. Both
processes no doubt play a part.

Up to the present, there has been little precise knowledge of
the structure of a simple flame. This has been partly due to the
great difficulty of obtaining a suitable flame on which to carry out
measurements.

3. *The Flat Flame.* During some work on the determination of
limits of combustion,[2, 4] Dr. J. Powling and I devised a novel type
of burner which gave an even distribution of velocity of flow
across its wide mouth, and provided the conditions for establish-
ing a plane flame front, such as that discussed in section 2. A
matrix formed of plane strip wound along with a corrugated strip
of cupronickel provides numerous evenly spaced channels in
which the flow is streamline. The flame takes up a position above
the rim of the burner and is a thin circular disc. Generally there
is a small turn up or turn down at the edge extending over a
distance of about a millimetre, but this is small compared with

the diameter of the disc which can be made as large as is convenient (for instance, 8 cms.).

A gauze, situated usually about 4 or 5 centimetres above the flame, not only prevents convection currents disturbing the flow conditions, but helps to reflect heat back to the matrix and this preheat (usually about 100°) is essential for the stability of the flame.

With heat provided in this way, stability is only obtained for slow flames (of mean velocity 3 to at most 15 cms/sec). Nevertheless, measurements of burning velocity near the limits of combustion are being obtained by this method[3] which promise good accuracy. Incidentally, the flame can be established and maintained for mixtures considerably weaker than the usually accepted limits mixtures.

Recently[1] this flame has been used by Mr. Weinberg in our laboratory at the Imperial College, South Kensington, to effect a determination of the temperature parameter throughout an ethylene-air flame by an optical method depending on the change of refractive index of the gases, and some progress is being made towards the study of the structure of the simple flame outlined in section 2.

4. With the flat flame Mr. Badami and I have been determining the burning velocities of mixtures of air with hydrocarbons from methane to butane, and now with mixtures of gases such as hydrocarbons with hydrogen and with carbon monoxide. It is interesting that the plane disc-like flame breaks up into the cellular structure under certain circumstances. These cellular flames have long been known, but they have been more closely studied recently by Markstein.[5] Using the flat flame burner, it is possible to study the change over from the flat flame to the cellular combustion. For instance, with a 1.9% ethylene-air mixture to which hydrogen is added, a change from 3.14 to 3.15% hydrogen will cause a small tip to form near the edge of the flame which on addition of a trace more hydrogen becomes a single cell directed towards the tip of the burner which proceeds to rotate round the flame edge — the sense of rotation can be changed by slightly altering the tilt of the burner. A trace more hydrogen causes the whole disc to break up into cells. With hydrogen and carbon monoxide mixtures we

have found that not only enriching with hydrogen, but also en-
riching with carbon monoxide produces the change from a flat
flame to a cellular flame. It is possible that the instability is not
necessarily associated with an increase in hydrogen atom con-
centration, although observations in general have been in accord
with such a hypothesis.

The maintenance of a stationary flat flame is dependent on the
balancing of the buoyancy of the gas layers against the force of
gravity. There is a pressure difference across the luminous flame
front, though very small. A perturbation can arise and can become
a permanent state, breaking up the front into cells. The flat flame
can only stabilise itself if lateral expansion can occur and this will
probably only be possible, not only if the preheat is such that
sufficient burning can occur at the existing flame front, but if
certain diffusion conditions are also satisfied. The conditions of
stability of the flame have yet to be clarified. Vibratory conditions
often arise in flames, more often in closed vessels, in which pressure
waves reflected back are shown to cause disturbance of the con-
ditions in the flame front. It is however interesting that a flame
can self-generate fluctuations and these cellular flames will repay
further study.

5. It is a difficult matter to investigate the fast chemical reactions
which occur in flames. Several methods have been used:

(a) *Sampling*, particularly using the mass spectrograph.

(b) *Spectroscopic* methods.

(c) *Sudden condensation.*

Our laboratory at South Kensington has employed the last two
methods. Dr. A. G. Gaydon's spectroscopic researches on flames,
and particularly flames at low pressure, are well known.[6] He has
shown that OH, CH and C_2 radicals are sometimes in exceptional
states of high rotational energy and that some substances intro-
duced into flames reach high levels of excitation. He has mapped
the region of appearance of these radicals and this has led to
knowledge of some of the reactions which provide radicals such
as CH and C_2. It is clear that in premixed flames thermal equi-
librium is often not complete amongst the products. Under certain
conditions in hydrocarbon flames, the CHO radical shows up
prominently and is generally associated with peroxidation.

Method (c), sudden condensation, has been used by Dr. Minkoff and Mr. Everett,[1] who have shown that quite considerable quantities of peroxides are formed in hydrogen-oxygen and methane-oxygen explosions at low pressure. These explosions at low pressure have been photographed. They take place at great speed, but in spite of that, from the yield of peroxide under different circumstances, it is possible to find out something about the reactions which occur in these fast flames. Hydrogen and oxygen explosions yield hydrogen peroxides of two kinds, the ordinary and one which appears to be unusually unstable and breaks up on warming to ordinary temperature. Methane-oxygen explosions yield some ozone and also some methyl hydrogen peroxide in addition to the hydrogen peroxide. The ozone probably arises from the combustion of the CO formed during the combustion of the methane. It is not yet known whether the methyl hydrogen peroxide (which is only about 0.1 % of the total peroxide) is a direct product of the oxidation or is only the result of subsidiary reactions.

Whatever reactions do occur when once the luminous boundary of the flame is passed, they occur with very great rapidity. The thickness of even a quite slow moving flat flame is a few tenths of a millimetre and for a large molecule like octane, much has to happen in a very short time.

It is possible to estimate for the flat flame how long there is between the moment when the temperature begins to rise ahead of the flame and the moment when luminous combustion first shows. The time is very small and in the case of methane it seems to be less than the delay for ignition of a like mixture of methane and air at 1,000°C. That the flame is maintained merely by conduction of heat into the unburnt gases does not therefore seem probable. There are good grounds for believing that radicals (hydrogen atoms chiefly) are also diffusing back to assist the initiation of the chain reactions.

6. In the above paragraphs, some of the ways have been mentioned in which simple premixed flames are being investigated. Parallel with this, the diffusion flame has been closely studied. Dr. Barr (University of Glasgow) and Drs. Parker and Wolfhard (Royal Aircraft Establishment, Farnborough) have made some interesting investigations. Dr. Barr has investigated the conditions controlling

the length of simple diffusion flames of butane in air over a very wide range of flows. Vibrating flames which are set up under some conditions appear to be due to enhancement of fluctuations in the fuel flow and not to a change in the nature of the chemical reaction in the flame.

Dr. Wolfhard[8] devised a diffusion burner of a special type which has made it possible to map closely by spectroscopic means the various radicals produced as the reaction proceeds in the various interdiffusing regions of the flame, and to relate this to the temperature distribution.

Mr. Rudrakanchana in my laboratory has been using the method to study the diffusion flames of organo-metallic substances. The object was to investigate the production of the solid products; the polymerisation to sizable particles. Just as with the formation of carbon particles in flames, the period is very brief. It is remarkable how quickly a solid product, such as carbon or lead oxide formed in a vapour phase chemical reaction, will even at great dilution polymerise to a solid particle in a disperse state. It is easily shown by the flat flame that the time available for carbon to form and then polymerise is only about one millisecond.

7. As mentioned earlier, in practise diffusion flames are often as important as premixed flames. In a spray of liquid particles heat has to be conveyed from particle to particle, and vapour to be distributed in air. If the particle is small enough, and mixing is rapid enough, the combustion behaviour may approximate to a premixed flame, but if the particles are larger or the mixing not rapid enough, the combustion behaviour will approximate to that of a collection of diffusion flames. The behaviour will be complicated by turbulence. Even these conditions are yielding to analysis.

8. I have attempted in this communication to indicate some of the directions in which investigations are proceeding on simple flames. Much valuable investigation is going on in many laboratories in other countries than my own and if I have not referred to such work, I plead the brevity of this report.

The object of all the work is for the better management of flame; in other words, in order to obtain such knowledge of flame behaviour that furnaces and other contrivances may be designed

so as to operate as required. We agree with Count Rumford that
" the advantages that may be derived from the Knowledge " of
the " communications of Heat by flame are of very great im-
portance and the subject deserves to be thoroughly investigated."

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Helium and the Range of Stability of the Solid State

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Much is known about the vapour-pressure curves which determine the gas-liquid and gas-solid boundaries in the equilibrium diagram (Figure 1), but of the melting-curve which defines the liquid-solid transition little can yet be said. Why is this?
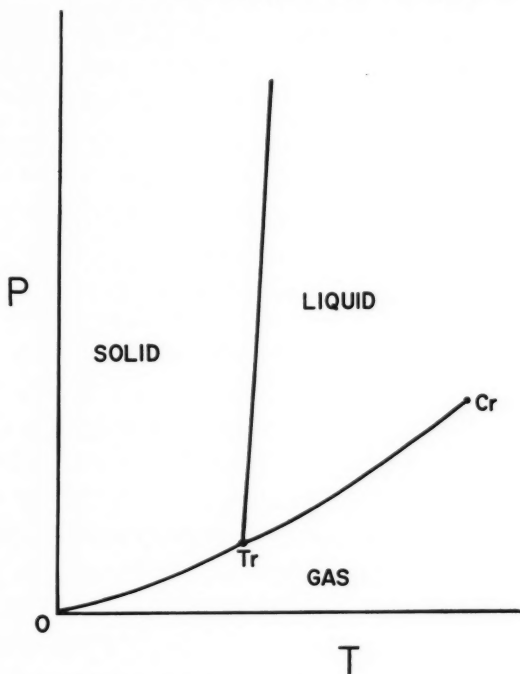
FIGURE 1. SCHEMATIC EQUILIBRIUM DIAGRAM OF THE THREE STATES OF AGGREGATION.

We can of course apply the Clausius-Clapeyron equation to both cases:

$$\frac{dp}{dT} = \frac{Q}{T\Delta V} \tag{1}$$

($Q =$ heat of evaporation or melting, $\Delta V =$ volume difference between the phases). For the two vapour-pressure curves we can — at least at the lower temperature end — replace ΔV simply by the volume of the ideal gas. In addition, we have quite a clear conception of the heat of evaporation and we also know that the temperature dependence of this quantity is not very strong, at least up to the normal boiling point. Letting this dependence vanish in first approximation, we arrive at the well-known equation:

$$ln\ p = -\frac{Q}{RT} + const. \tag{2}$$

In the case of the melting curve, however, both phases are in a very dense state and no obvious " ideal " laws can be applied. No general explanation of the volume change or of the heat of melting can be proffered, nor can we predict their dependence on temperature; consequently, it is not possible to integrate the Clausius-Clapeyron equation. We can only say that the melting curve must always be extremely steep because of the very small volume difference between the two phases of aggregation. In fact, even the highest pressures which Bridgman[1] could apply have, in the case of the substances melting at normal temperatures, failed to raise the melting temperature much above the temperature of the ordinary gas-liquid critical point.

Without further theoretical guidance most observers have in these circumstances rightly refrained from attempting to generalize their results with the exception of Tammann,[2] who fell into the familiar trap of extrapolating interpolation formulae valid over only a very limited range. His prediction that the melting temperature would rise to a maximum and then fall with rising pressures, so that the solid state would form a finite enclave, has, however, unfortunately dominated the ideas of some geophysicists for a long time.

A knowledge of the behaviour of the melting curve at very high pressures and temperatures is not only desirable because of its intrinsic interest for the condensed state but also for geophysical applications. I therefore tried some time ago to find out

whether the parameters of the melting curve could not be related to some general physical property of either of the phases. In order to do this I tried first to find out[3, 4] from the experimental material — which we owe mainly to Bridgman — how a suitable dimensionless quantity changes along the melting curve. The quantity $\frac{r}{p\Delta V} = \frac{d\ ln\ p}{d\ ln\ T}$ ($r =$ heat of melting) was chosen and all the known melting data were plotted in a $log\ p - log\ T$ diagram. It then turned out that all melting curves become straight lines at the higher temperatures and that in addition most of them are parallel to each other. Naturally, in the region of the normal melting point — that means at very small pressure — the linear correlation must break down, as the log of the pressure must approach minus infinity at a finite melting temperature. This, however, is only to be expected. The pressure zero does not command a position of particular significance in condensed systems, in contrast to gases, and even negative pressures are perfectly possible because of the cohesion of condensed phases. It was then tried whether the simple addition of a term representing an appropriate internal pressure (a) would improve matters. Indeed it was then found that for every substance an a could be chosen which would render the relationship between $log\ (p + a)$ and $log\ T$ linear. Thus the formula:

$$log\ (p + a) = c\ log\ T + b \tag{3}$$

resulted or, equivalently,

$$\frac{p}{a} = \left(\frac{T}{T_0}\right)^c - 1 \tag{4}$$

($T_0 =$ normal melting temperature). Furthermore, it was found that the a's determined in this way agreed very well with the internal pressures calculated as $\left(\frac{\partial U}{\partial V}\right)_T$ using van der Waals' equation. As mentioned before, the curves were nearly parallel, which means that the constant c has about the same value (about 2) for all substances except the metals.

These features gave us sufficient confidence to use this formula in planning our experiments. Given a certain limit of physical pressure, equation (4) predicts that one should employ a substance of small internal pressure in order to penetrate to relatively

high pressures and temperatures. Now the internal pressures of the substances melting at ordinary temperatures are of the order of 5,000 atmospheres, while substances of low internal pressure will be those with weak interatomic forces, that is, substances having low boiling points. Thus from van der Waals' equation one can estimate that for helium a should be of the order of 20 atmospheres. This means that by using helium instead of a substance melting at normal temperature, we would have in our favour a factor of 250 in the pressure. Experiments[5] on helium soon showed that even at the boiling point of hydrogen, i.e., at four times the critical temperature of helium, it could be solidified under rather moderate pressures (2000 atmospheres), and in later experiments the author and his collaborators extended these measurements to 40° abs.[6]

Of course, in considering helium, one must take account of the anomalous behaviour of this substance at very low temperatures: helium does not solidify at all under its saturation pressure and therefore has no triple point. However, the change over from normal to anomalous behaviour takes place very abruptly at the λ-point*; the melting curve at higher temperatures can be regarded as a curve for an ordinary substance with a normal melting temperature of about 1° abs.

Apart from helium and hydrogen, neon, nitrogen, and argon were also investigated[8] and Table I shows that in accordance with the prediction of equation (4), a given external pressure (5,000 atmospheres) raises the melting point $T_{5,000}$ to a *relative* temperature ($T_{5,000}/T_{cr}$) which is the higher the lower the critical temperature (T_{cr}) of the substance.

TABLE I

	T_{cr}	$T_{5,000}/T_{cr}$
He	5	7.3
H_2	33	2.3
Ne	45	1.7
N_2	126	1.0
C_6H_6	563	.7

* See Simon and Swenson.[7] In this paper we also discuss the significance of the melting curve of helium near absolute zero.

Thus the semi-empirical formula was quite successful and, in fact, even workers who did not believe in the significance of the internal pressure term used it to represent their experimental results.

New Experiments

In the last few years we have started up work again in this field. We have made further experiments and have investigated in more detail both the meaning of the reduced formula and the extent to which we can use helium as a model substance. We recall that in the last few years attempts have been made by Domb[9] and de Boer[10] to derive the melting formula from first principles. Using the approach of Lennard-Jones and Devonshire,[11] they have succeeded in doing this for the region of high temperatures and pressures; in particular de Boer was able to show that the constant a really corresponds to the internal pressure. The model used by these authors had of necessity to be oversimplified and therefore it was not possible to derive an accurate expression for the constant c. Also this model could not really be applied to a degenerate substance like helium.

Very recently a more general derivation of equation (4) has been given by L. S. Salter[12] of the Clarendon Laboratory. As we mention later on, our new experiments show that the Lindemann melting point formula is valid for solid helium over a very considerable range of temperatures and pressures. Salter combined the Lindemann formula with Grueneisen's equation of state, and showed that the melting formula follows immediately and that a corresponds strictly to the internal pressure. This deduction is, moreover, not restricted to the classical region. Although solid helium is in a highly degenerate state and although zero point energy plays a very big role in both the solid and liquid phases, Salter's treatment, together with the previous experimental knowledge that the melting curve for helium can be reproduced over the whole range by formula (4), is a justification for using helium as a model substance for the process of melting. (The reason why helium obeys a reduced formula in spite of quantum effects is that their main influence is on the internal pressure, and the parameter a automatically takes care of this. If, however, we follow de Boer[13] and use molecular constants as reducing parameters,

then large deviations from the law of corresponding states will occur at low temperatures.)

Before describing the new experiments, a word about solid helium which may appear to some as a rather unusual solid. Solid helium is a very hard substance which can block capillaries against thousands of atmospheres of pressure difference. Keesom and Taconis[13] have shown that it has a normal type of close packed hexagonal structure and we have even observed a crystallographic transition in it. (See below.) The melting transition is very sharp and the liquid in equilibrium with the solid possesses a very low viscosity.[14] Webb and Wilks[15] have shown that solid helium has a thermal conductivity like that of other normal crystals and that the helium generally solidifies as a single crystal.

Our new experiments on the melting curve of helium were concerned with two particular problems. Firstly, we wished to extend the melting curve to higher temperatures and pressures. By adopting different techniques from those used by Simon, Ruhemann and Edwards,[6] and by Holland, Huggill and Jones,[16] Dr. Robinson[14] has succeeded in carrying out experiments with pressures up to about 10,000 atmospheres, when helium melts at about 60° abs. (Figure 2). Similar experiments were also done with argon, the melting point of which is raised by a pressure of 10,000 atmospheres to 240° abs. As in the case of helium these experiments give good confirmation of equation (4). On extrapolating this equation one finds that helium at room temperature would be a solid at pressures above 100,000 atmospheres. This, of course, only holds if a solid-fluid critical-point does not intervene before room temperature is reached.

The second problem was to find whether there is any trend along the melting curve towards such a critical point. In order to do this we measured the entropies of solid and fluid helium along the curve; an approach to a critical point would show itself in the fact that the entropies of both phases tend to become equal. It would be extremely difficult, if not impossible, to measure the specific heats of normal substances at high pressures as the heat capacity of the high pressure container would overshadow everything else. Here again the use of helium as a model substance has a decisive advantage. At the temperatures concerned, steel has a specific heat which is quite negligible and we are able to work more or less
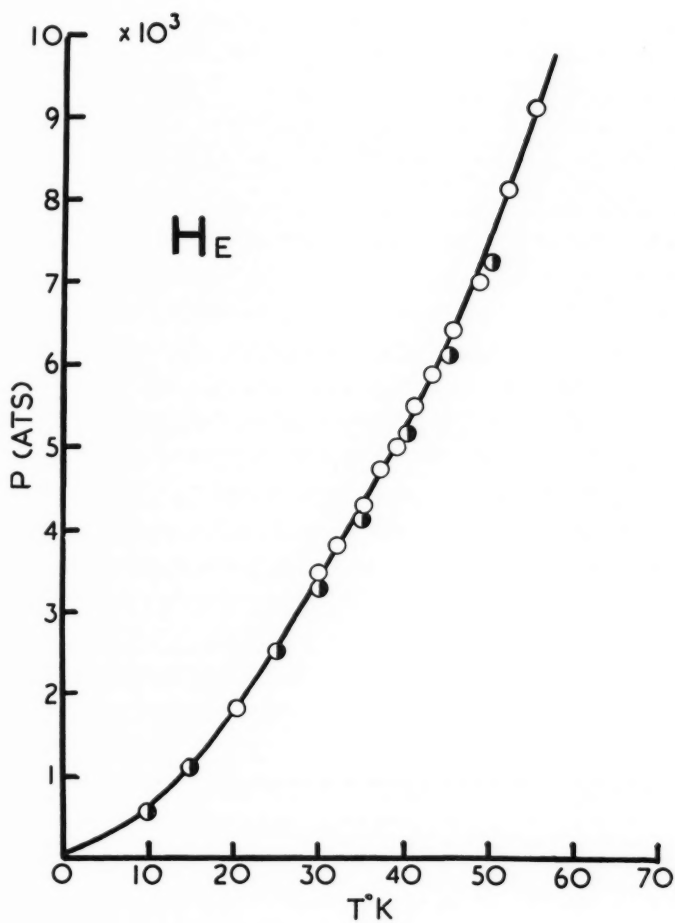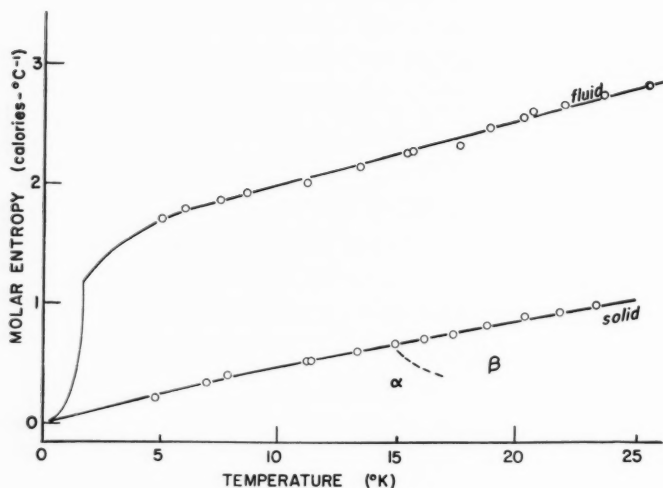
FIGURE 2. MELTING CURVE OF HELIUM.

FIGURE 3. ENTROPY OF SOLID AND FLUID HELIUM ALONG THE MELTING CURVES.

with mathematical walls. It was therefore possible for Dr. Dugdale to measure the specific heats of highly compressed solid helium and the heats of melting. By applying the third law of thermodynamics one can then construct the whole entropy diagram which is reproduced in Figure 3. These experiments have so far been carried out up to 3,000 atmospheres corresponding to temperatures slightly below 30° abs. Details of the experiments, together with data on the equation of state of solid helium, will be published elsewhere;[17] here I will only mention those points that concern the melting curve.

The entropy of melting turns out to be of the order of the gas constant *R*, as with most other substances. The fact of greatest interest to us, however, is that the entropy differences do not diminish but rather increase slightly with rising temperature. It should be noted that Figure 3 gives data for temperatures five times greater than the critical temperature, a region in which it has not been possible to measure the properties of any other substance. From the figure it also follows that the heats of melting, in the range of temperature investigated, are about three times higher

than the heat of evaporation of liquid helium at absolute zero. This again emphasizes the fact that the differences between the two phases are very pronounced and that the difference is increasing with temperature.

As we see in Figure 3 there is a transition of the first order within the solid state. As it is impossible to see how crystalline helium could exist in any arrangement other than a close packed one and as we know that the low temperature (α) phase is hexagonal close packed, we have to conclude that the high temperature or β phase is cubic close packed. As would be expected from this explanation, the differences in energy and volume are very small and the break in the melting curve is quite invisible.

The Lindemann Formula

The volume of solid helium at 3,000 atmospheres is only 40% of its value at absolute zero. The characteristic temperature at this volume, derived from the specific heat, is about five times higher than its value for helium under the melting pressure at absolute zero. We are therefore in a good position to test the Lindemann melting point formula which connects the melting temperature with the Debye characteristic temperature θ, the molar volume and the mass of the atom:

$$\theta = const. \sqrt{\frac{T_m}{MV^{2/3}}}$$

The following table gives the data for a few selected points:

T_m	V	θ	$const.$
23.3	10.6	110	101
17.3	11.6	92	100
11.3	13.1	72	102
7.9	14.4	55	95
3.1	18.3	32	96

We see that the constant in the formula changes only by a few per cent in spite of the very great range of the other parameters. This is a rather surprising result which has been discussed in more detail by Domb.[18] The main interest for us at the moment is that it gives a justification for Salter's derivation of formula (4).

Conclusions

Summing up the results, we should mention several points. First, it has been possible to observe melting at temperatures over fifty times the normal melting temperature. Since substances other than helium agree well with the reduced melting formula, we may therefore generalize this statement to cover all substances with similar binding forces. The usual equilibrium diagram shown in Figure 1 represents the data from a much too restricted view;
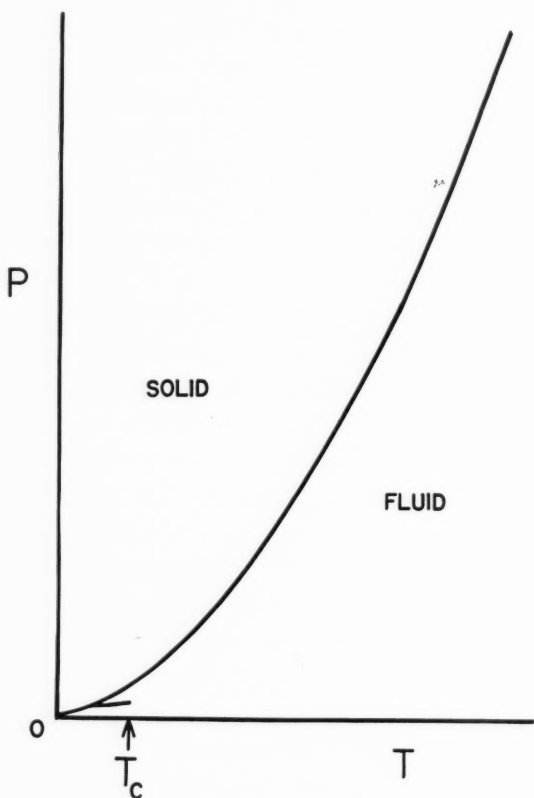

FIGURE 4. EQUILIBRIUM DIAGRAM BETWEEN SOLID AND FLUID STATES.

Figure 4 gives the diagram in a new perspective. The real dividing line is that between the crystalline phase and the fluid. The liquid-gas equilibrium is really only a very small appendix which has so far been over-emphasized because we happen to live under practically zero pressure.

The second point is that even at these very high temperatures there are no indications of our approaching a critical point. Of course, if we were to use pressures of a few hundred thousand atmospheres, then the outer electrons of the atoms would be squeezed off, and it is not possible to foresee how this would influence the states of aggregation.

Third, as we see in Figure 3, the entropy of the solid increases continuously along the melting curve with rising temperature. We had previously deduced this fact from some calculations on the alkali metals,[4] but these data did not have the same degree of certainty as have the direct measurements on helium. That the entropy curve along the melting line increases with rising pressure is not a matter of course; from the thermodynamic point of view it could just as well fall. The fact that it rises means that the effect of temperature in increasing the entropy is bigger than that of rising pressure which reduces the entropy.

This fact has one important consequence in that it shows the impossibility of melting a substance by adiabatic reversible compression. If we move along an isentropic line to higher temperatures, we move more and more into the range of stability of the solid phase. The question arises whether this is a general property of matter or only of the particular substances mentioned above. Salter's work can tell us something about this. We find that the entropy along the melting curve will always rise with increasing temperature (i.e., T/θ increases) if the quantity γ, which is defined by

$$\gamma = - \frac{d \ln \theta}{d \ln V} = - \left(\frac{\partial \ln T}{\partial \ln V} \right)_S$$

is bigger than $2/3$. For actual crystals γ has values between 1.5 and 2.5; the value $2/3$ would actually correspond to an ideal gas. The statement that one cannot melt a crystal by adiabatic reversible compression seems therefore to have a wide range of validity. It is true that one cannot make predictions for some abnormal substances like those possessing a very loosely packed lattice, but,

as Bridgman has demonstrated, even these substances are forced into more normal behaviour by high pressures.

I hope to have shown that "model" experiments with the low boiling substances can give information of general importance that cannot be obtained easily in any other way. We are now trying to extend our experiments to appreciably higher pressures, as well as to some other low boiling substances, in particular to hydrogen and its isotopes and to solutions of helium with hydrogen.

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Positronium

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It is rather striking that the experiments which have attracted the greatest attention of physicists in recent years are largely of two extreme types: they deal either with very high or with very low energies. Scattering experiments with almost all known particles are constantly extended to higher energies. Some of the important results obtained are described in the summary of the paper by Dr. Fermi. On the other hand the technique of radio-frequency spectroscopy is applied to transitions between nearly degenerate atomic levels. Some of the most beautiful and significant experiments of this type deal with the fine- and hyperfine structure of hydrogen discussed by Dr. Lamb and Dr. Purcell.

The reason for this dual interest in very high and very low energies is our increasing concern with the details of the forces between " elementary " particles, especially at short distances. If short-range forces are to have an important effect on the outcome of scattering experiments, the particles must have sufficient energy so that their deBroglie wavelength is comparable with the range of interest. Alternatively low energy experiments can be carried out with bound systems and may reveal the small remaining contribution at short range since they permit very high precision, especially if conditions are so chosen that the longer range interaction cancels in the result.

A detailed study of the interaction between two electrons is of special interest since according to present theory it should involve only electromagnetic phenomena which are believed to be fully understood, at least in principle. Electron scattering experiments at high energies are difficult since at the extreme velocities required by the low mass of the particles billion-volt energies involve a relative motion of the two electrons characteristic only of million-volt phenomena. On the other hand the observation of inter-electron forces in atomic spectra is always perturbed by the dominant effect of the nucleus. For this reason we have undertaken

the study of the bound state of two electrons – one positron and one negatron – which does not contain a nucleus.*

The possibility of a bound positron-electron system analogous to the hydrogen atom seems to have been first discussed in the literature by Mohorovicic (1934), who suggested that its optical emission lines might be observed in stellar spectra. Ruark (1945) suggested the name " positronium," now universally applied to this structure. Wheeler (1946) analyzed some of the properties of positronium and of systems consisting of several positive and negative electrons. More detailed theoretical considerations have been presented, e.g. by Pirenne (1947), Berestetzki (1949), Ferrell (1951).

The gross structure of positronium states is exactly like that of hydrogen except for the effect of the different reduced mass of the electron, which reduces all energies by a factor 2 compared with a hydrogenic atom with an infinitely heavy nucleus. Thus the energy of the n'th state is $E_n = - e^4 m_0 / 2\hbar^2 n^2$; the ionization potential is 6.8 volts, the energy of the first excited state is 5.1 *ev,* the Lyman a line has a wavelength of about 2,400 A, etc. The electron wave functions in the center-of-mass system are, in this approximation, the same as for hydrogen. The negatron-positron distance, which is usually the relevant coordinate, is, however, twice as large as the electron-proton distance; hence the statement that the positronium Bohr radius is twice as large as the corresponding hydrogen radius.

The symmetry arising from the equality of the masses of the two particles does not impose any conditions connected with the Pauli exclusion principle since the particles are distinguishable. The transition probability for optical (dipole) transitions is half as great as for the corresponding hydrogen lines. The transition dipole moment $<ex>$ is twice as large, the emitted frequency v is half as great, and the transition rate is proportional to $<ex>^2 v^3$.

These features of the gross structure are fairly obvious and there can hardly be any doubt that the optical spectrum of positronium will be observed in accordance with these predictions when favorable experimental conditions can be found.

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*Following the accepted practice, we shall refer to positively and negatively charged electrons as positrons and negatrons, respectively.

Of greater fundamental interest is the fine structure of the levels. This has been studied theoretically by Pirenne (1947), Berestetzki (1949) and Ferrell (1951). Karplus and Klein (1952) have investigated the effects of radiative corrections to the fine structure. The main differences between the fine structure of positronium and that of hydrogen are the following: 1. There is a relativistic orbit-orbit interaction between the particles which is negligible in the case of the slow-moving proton. 2. The magnetic spin-spin interaction between the particles is of the same order as the fine structure while in hydrogen this hyperfine structure is smaller in the ratio of the magnetic moments of proton and electron. It is thus appropriate to speak of triplet (ortho) and singlet (para) states of positronium. 3. There is an additional spin-dependent interaction arising from the possibility of virtual annihilation and re-creation of the pair. This can take place only when the particles coincide and has therefore the character of a short-range force which is of importance in S-states only. Since energy is not conserved in the virtual intermediate state, one-quantum annihilation which can conserve angular momentum in the $^3S_1$ state yields the most important contribution. This "annihilation force" has also been considered by Bhabha (1936) in connection with positron-negatron scattering.

Of particular interest is the splitting between the triplet and singlet components of the $1s$ ground state. This splitting is due to two terms. 1. The magnetic spin-spin interaction which gives rise to the Fermi-Segré term in the hyperfine structure of hydrogen contributes an energy $-8\pi\mu^2\psi^2(o)$ to the singlet and $+8\pi/3\mu^2\psi^2(o)$ to the triplet state, making the total magnetic splitting $(32\pi/3)\mu^2\psi^2(o)$. Here μ is the magnetic moment of the electron and $\psi(o)$ is the magnitude of the electron wave function at the position of the positron. 2. The annihilation force contributes an energy $+8\pi\mu^2\psi^2(o)$ to the triplet state. Thus the total splitting of the ground state as $(56\pi/3)\mu^2\psi^2(o)$. Substituting $\psi^2(o) = 1/\pi(2a_0)^3$ and $\mu = \mu_0 = e\hbar/2mc$, we obtain for the splitting ΔW,

$$\Delta W = (7/3)\mu^2_0/a^3_0 = 8.45 \times 10^{-4}\text{eV} = 2.044 \times 10^5 \text{ Mc/sec} \quad (1)$$

Thus the ground state of positronium is split into two levels in the same manner as the ground state of the hydrogen atom. As in the case of hydrogen, radiative transitions between these states

are highly unlikely and we may expect the singlet level (para-positronium) and the triplet level (ortho-positronium) to be populated very nearly according to their statistical weights, i.e. in the ratio 1 : 3. The most fruitful result to date of the investigation of the properties of positronium has been a rather precise and only slightly indirect determination of ΔW.

Before discussing these measurements, I want to outline briefly how positronium is produced and how we detect it. The most important property of positronium in this connection is its annihilation. A positron-negatron pair even in a free state is unstable against transformation into electromagnetic radiation. The result of this annihilation transition is the disappearance of the negatron and the positron and the emission of electromagnetic radiation, with total energy $E_\gamma = 2mc^2 + E_+ + E_-$. E_+ and E_- are the energies (kinetic and potential) of positron and negatron, respectively, in excess of their rest energies.

The electromagnetic energy E can be emitted as a single quantum only if a sufficiently strong external field is present to absorb the momentum. Otherwise conservation of momentum requires the emission of at least two quanta. In the absence of special restrictions, to be discussed later, two-quantum annihilation is the most probable fate of a positron in matter. The two quanta are always emitted in opposite directions with equal energy in the coordinate system in which the center of mass of the two electrons is at rest. The kinetic energy of the two electrons just before annihilation is usually small compared with mc^2. In this case the two quanta have energies of very nearly mc^2 each and are emitted in opposite directions in the laboratory system.

This characteristic 0.51 Mev annihilation radiation was observed very soon after the discovery of the positron, and the emission of two quanta, predominantly in opposite directions, was demonstrated by coincidence measurements.

Two-quantum annihilation is " allowed " only for an electron pair state of complete spherical symmetry, i.e. a $^1S_0$ state. In the language of classical mechanics this means that the two electrons must meet " head-on " (i.e. with zero orbital angular momentum) and with their spins " anti-parallel " (i.e. in a singlet state). When two-quantum annihilation is forbidden, three quanta are emitted. In the absence of external fields, two- and three-quantum annihila-

tion, respectively, are possible only from states of opposite parity and therefore never compete.

The probability of two-quantum annihilation was first calculated by Dirac (1930), in terms of an electron cross-section σ. For non-relativistic velocities this cross-section is:

$$\sigma = \pi r^2_0 c/v \qquad (2)$$

where v is the relative velocity of negatron and positron, and r_0 is the classical electron radius. The rate of annihilation λ in a medium containing n electrons per cc. is then

$$\lambda = \sigma n v = \pi r^2_0 c n = 7.50 \times 10^{-15} n \text{ sec}^{-1} = 4.52 \times 10^9 \ \rho Z/A \text{ sec}^{-1} \qquad (3)$$

ρ, Z and A are the density, atomic number, and atomic weight respectively of the medium. The mean life of a positron of non-relativistic velocity is then given by $\tau = 1/\lambda$.

When the annihilation by two-quantum emission is ruled out by the symmetry of the positron-negatron wave function, three-quantum emission is the process of lowest order possible. This is true specifically in ^3S states which contribute three quarters of all the collisions at low energy.

Three-quantum annihilation is characterized by a lower characteristic decay rate. Ore and Powell (1949) have calculated the ratio of the three-quantum to two-quantum rate and find

$$R_{3q}/R_{2q} \ \frac{4}{9\pi} \ (\pi^2 - 9)a \simeq \frac{1}{1120} \qquad (4)$$

In the above discussion we have tacitly assumed the negatrons to be completely " free," i.e. we have neglected the electrostatic attraction between them and, for electrons bound in atoms, the repulsion of the nucleus for the positron. These effects depend on the positron energy but they will in general not change the order of magnitude of the result.

Equation 3 leads to life-times of the order of 10^{-9} to 10^{-10} seconds in typical solids, depending on the Coulomb effects considered. In gases at atmospheric pressure the life-time is of the order of 10^{-7} seconds; again this value has to be modified by a Coulomb correction. Whatever the exact effect of the correction, we can be sure of two characteristics of the annihilation of positrons in " free " collisions: 1. The decay rate, i.e. the reciprocal of the life-time,

must be proportional to the electron density and therefore to the gas pressure in any one gas (Eq. 3). 2. Three-quantum annihilation occurs only for about one in 370 positrons (Eq. 4). A factor of three multiplies the ratio given by Equation 4 because of the relative statistical weights of singlet and triplet collisions.

The situation is different if annihilation occurs in a bound positronium state. The electron density n (Eq. 3) is now the density in the positronium atom rather than the effective average density in the gas. This density is, of course, independent of the gas pressure and is of the same order as that in solids. Specifically, in the ground state of positronium (both ortho- and para- forms), it is $n = 1/\pi a^3$, where $a = 2a_0 = 1.06 \times 10^{-8}$ cm. a_0 is the first Bohr radius. Substituting this into Equation 3 we get for the singlet (para) state a decay rate of $\lambda_p = 0.8 \times 10^{10}$ sec^{-1}, or a mean life $\tau_p = 1.25 \times 10^{-10}$ sec. For the triplet (ortho) state Equation 4 yields $\lambda_0 = 0.7 \times 10^7$ sec^{-1} or $\tau_0 = 1.4 \times 10^{-7}$ sec. Thus, if all positrons formed positronium in a gas, we should expect to observe two components in the decay: a very short-lived one, due to the singlet state, containing one quarter of all the annihilations, and one with a mean life of 1.4×10^{-7} sec containing the remaining three quarters. This latter period should be independent of gas pressure provided the ortho-positronium is stable against collisions with gas molecules. Furthermore, three quarters of the annihilations should proceed by three-quantum emission, resulting in a continuous gamma ray spectrum. Annihilation without positronium formation results overwhelmingly in the emission of two mono-energetic 510 kev gamma rays.

In practice only some of the positrons form positronium, about 30 per cent under usual conditions. In some gases the ortho-positronium is rapidly destroyed by collisions. This is true for NO, NO_2, the halogens and, to a lesser extent, for oxygen. In some other gases, notably freon, the annihilation of " free " positrons proceeds so rapidly that only positrons forming ortho-positronium survive longer than about 10^{-7} sec. Figure 1, taken from a paper by the author (Deutsch 1951), shows the annihilation rate of positrons in oxygen and freon as a function of gas pressure. Oxygen shows the proportionality of decay rate with pressure, characteristic of annihilation in free collisions, while freon shows the pressure-independent decay of ortho-positronium.
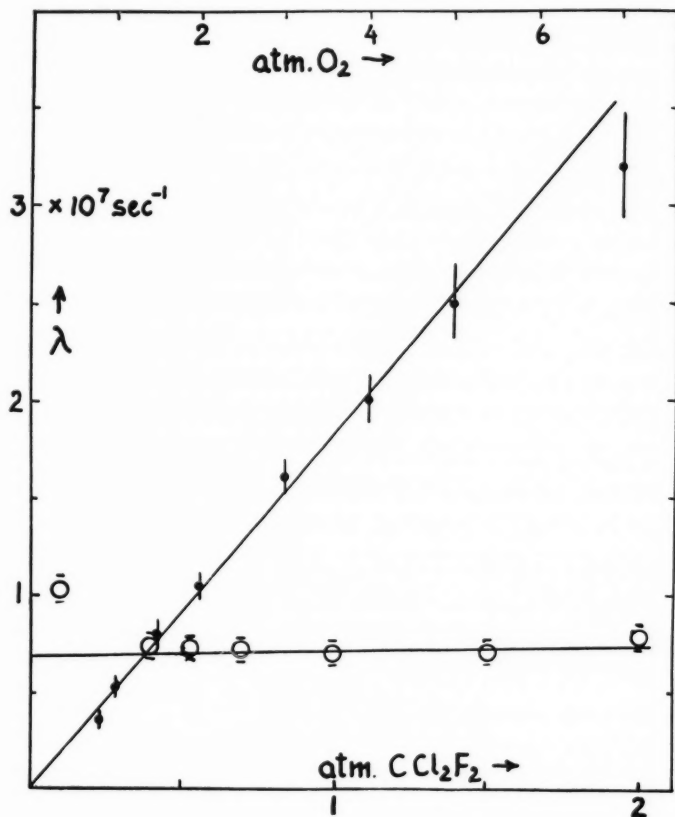
FIGURE 1

These results were obtained by measuring the time intervals between the emission of nuclear gamma rays in the radioactive decay of Na^{22} and the emission of annihilation radiation due to positrons annihilated in the gas. Since the positrons are emitted practically simultaneously with the nuclear gamma ray, the distribution of time intervals is a direct representation of the rate of annihilation of positrons after their creation in beta decay.

Another method — in many respects more convenient — for detecting positronium is based on the measurement of the energy spectrum of the annihilation gamma rays. Two-quantum annihilation gives rise to a " line " spectrum of 510 kev radiation while three-quantum decay, characteristic of the triplet state, yields a continuous gamma ray spectrum. A measurement of the relative intensity of the continuum and the 510 kev line therefore indicates the probability of annihilation in a triplet state. Such a measurement is relatively easy by means of a scintillation gamma-ray spectrometer, and this is the method adopted in our experiments. Other methods have been employed by other workers (Pond 1952, Wheatley and Halliday 1952).

After establishing the abundant formation of positronium in gases and after preliminary studies to establish favorable conditions for the experiments, we proceeded to investigate the fine structure of the ground state. To understand these experiments, consider the Zeeman effect of the ^1S and ^3S levels as illustrated in Figure 2. It is a remarkable property of positronium that none of its states shows a linear Zeeman effect, i.e. none of them has a permanent magnetic moment. This is rather easy to understand in terms of a semi-classical model: since any orbit is equally traversed by a negatron and a positron, there is no net current associated with the orbital motion and therefore no orbital magnetic moment. The spin magnetic moments have opposite signs with respect to the spin angular momentum for positron and negatron. Thus the moments cancel in triplet states. In singlet states, which are characterized in this picture by an " antiparallel " orientation of the spins, one might expect a net magnetic moment since the two moments should be parallel. However, since there is no preferred direction in this state, the moment will fluctuate rapidly and its average observed value is again zero.

While positronium has no permanent magnetic moment, there may be an induced moment in a magnetic field and therefore a second order Zeeman effect. Quantum mechanically this is due to the " mixing " of states with equal quantum numbers L and m_s but different total spin S. The sub-levels with $m = \pm 1$ of the triplet state are completely unaffected by the magnetic field since there are no $^1S_0$ components of the same m with which they could combine.
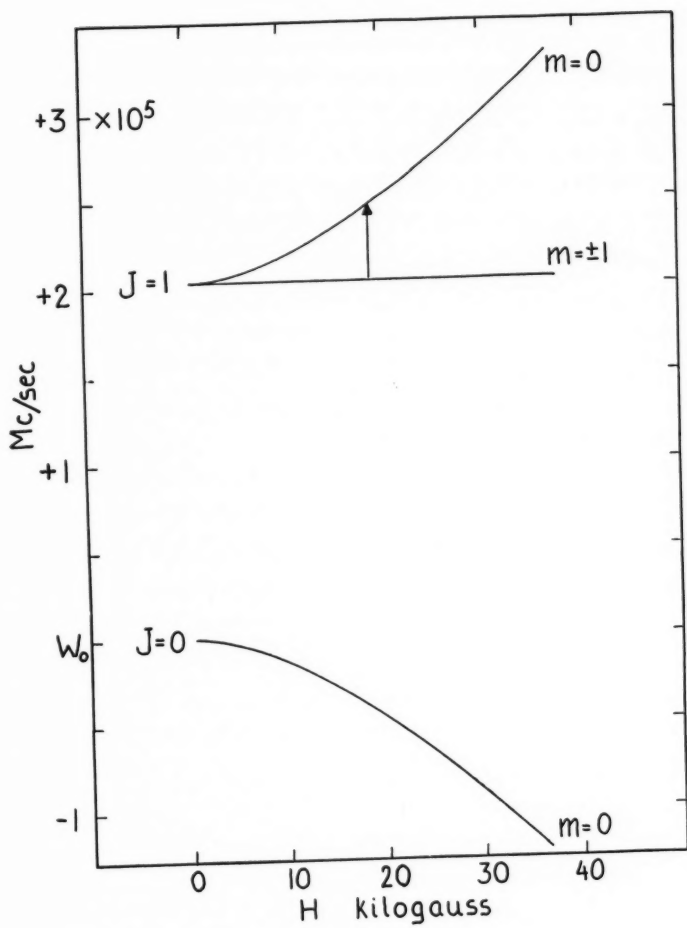
FIGURE 2

On the other hand the sub-state with $J = 1$, $m = 0$ can mix with the singlet level. In a very strong field two eigen-states of the system can be described by stating that in one of them the positron spin points in the direction of the magnetic field and the negatron spin in the opposite direction, while in the other state these directions are reversed. Each of these states is a mixture of equal contributions from the singlet and triplet components with $m = 0$. In this high-field (Paschen-Back) region the magnetic energy is proportional to $2\mu H$. In low fields the admixture of singlet to the triplet wave function (or vice versa) is given by

$$a = (x/2) \ (1 + x^2/4)^{-1/2}, \ x = 4\mu H/\Delta W \qquad (5)$$

ΔW is the singlet-triplet splitting (Equation 1). The magnetic energy w is approximately

$$w = \pm 2\mu H a = (2\mu H)^2/\Delta W; \ a << 1 \qquad (6)$$

i.e. the Zeeman effect is quadratic. As we see in Figure 2, the sign of the effect is such that the two states " repel " each other as is usual with second order perturbations. A more accurate expression for w can be obtained without resort to perturbation procedures. The expression is essentially the Breit-Rabi formula. The energies of the levels with respect to the unperturbed singlet state are

$$W - W_0 = (\Delta W/2)\left[1 = mx \pm (1 - 2mx + x^2)^{1/2} \right] \qquad (7)$$

(c.f. Figure 2). One of the consequences of the application of a magnetic field is the admixture of a certain amount of singlet wave function to the triplet state with $m_s = 0$. One may say that the atoms, which in the absence of the field were in this state, now spend a fraction of the time $a^2 = (2\mu H/\Delta W)^2$ as a singlet. But the singlet state can be annihilated by two-quantum emission at a rate 1100 times faster than the decay of the triplet. Thus, in a very strong magnetic field all of the $m_s = 0$ atoms (one third of the ortho-positronium) will decay by two-quantum instead of three-quantum annihilation. In weaker fields the three-quantum emission is partly quenched, the fraction ϕ remaining in a field H being

$$\phi = \frac{\lambda_0/\lambda_p + a^2(1 - 1/3)}{a^2 + \lambda_0/\lambda_p}.$$ The factor $1/3$ is modified somewhat

for some methods of observation by the angular distribution of the radiations with respect to the direction of the magnetic field.

Deutsch and Dulit (1951) observed ϕ as a function of H. Assuming λ_0/λ_p to be given by Equation 4, we used ϕ to determine a^2 and, from this, ΔW. Although the determination was rather rough, it showed clearly the existence of the " annihilation force " term. Wheatley and Halliday (1952) repeated these experiments with slightly better accuracy. Assuming ΔW to have the value determined by Deutsch and Brown in an experiment to be described below, they used their results to calculate λ_0/λ_p. Good agreement with Equation 4 was found. Taken together with the experimental determination of λ_0 by Deutsch (1951), this work of Wheatley and Halliday also establishes an experimental verification of Equation 3.

While these experiments determined ΔW with an accuracy of about $\pm 10\%$, it seemed desirable to measure this quantity with high precision. The obvious procedure would be to induce transitions from the triplet to the singlet state by means of a radio-frequency field and to observe the resulting reduction in the three-quantum decay. Unfortunately the required high radio-frequency fields (several gauss) are at present not available at the frequency of this transition (2×10^5 Mc/sec). We therefore decided to measure instead transitions between the $m = \pm 1$ and $m = 0$ components of the triplet, indicated by the arrow in Figure 2. The frequency of this transition can be calculated from Equation 7 and a suitable choice of H brings it into the range of the available source of radio-frequency power. In a field of about 10,000 gauss the transition frequency is about 3,000 Mc/sec. The experiment was designed as follows: A positron emitting source was deposited at one end of a resonant cavity located in a uniform magnetic field of about 10^4 gauss. The cavity was excited by a magnetron in a suitable mode. A scintillation spectrometer analyzed the gamma ray spectrum arising from the positrons annihilated in the gas filling the cavity, while the magnetic field was varied in the neighborhood of the resonance value given by Equation 7. In a magnetic field of this magnitude the $m = 0$ state decays almost entirely by two-quantum annihilation, while the $m = \pm 1$ states decay entirely by emission of three quanta. Thus, at resonance, one expects to observe a reduction of the continuous gamma ray spectrum compared with the 510 kev line.

The results of Deutsch and Brown (1952) did indeed show this
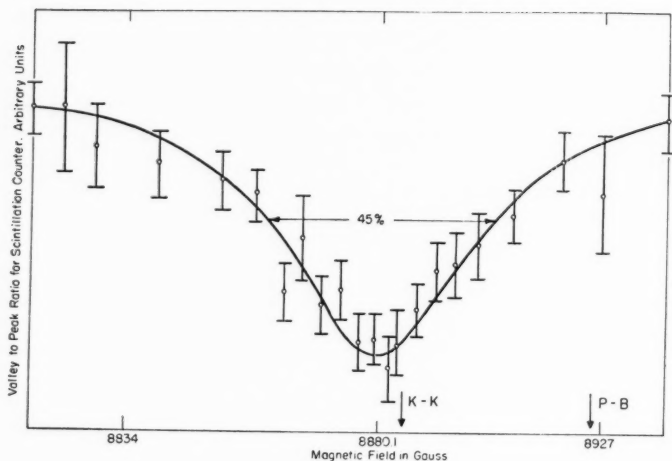
FIGURE 3

phenomenon and yielded a measurement of the ground state split-
ting, ΔW. Figure 3 shows a typical curve, more recent than the
published results of Deutsch and Brown, of the three-quantum
intensity, in arbitrary units as a function of the magnetic field near
the resonance, with a radio-frequency field of about 3,000 Mc/sec.
The arrow marked *P-B* shows the position of the resonance ex-
pected from Equation 1, in clear disagreement with the experi-
mental result.

Equation 1 holds only to order a^2 with respect to the gross
structure. Karplus and Klein have calculated the splitting to
the order a^3. The corrections involved arise, e.g., from the
effect of the "anomalous" magnetic moment of the electron
$\mu_e = \mu_0 (1 + a/2\pi)$ on the magnetic interaction, from virtual two-
quantum annihilation in the singlet state, from zero-point fluctua-
tions of the electromagnetic field, etc. As a result Equation 1 is
replaced by

$$\Delta W = (\mu^2_0/a^3_0)\left[7/3 - (32/9 + 21n2)a/\pi \right] = 2.0337 \text{ Mc/sec. (8)}$$

The value of the resonance magnetic field derived from Equation
8 is indicated in Figure 3 by the arrow marked *K-K* and is in

satisfactory agreement with experiment. The small remaining dis-crepancy is within the experimental and theoretical uncertainties. Experiments are now under way to increase the precision of the measurements. In the meantime we can conclude that the pre-dictions of the new quantum electrodynamics are in agreement with the experimental results concerning the interaction between electrons — an agreement within the already quite respectable ac-curacy of the experiments.

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Excited Hydrogen Atoms

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One might divide spectroscopic methods into two classes. In the first, the physical substance of interest emits light which is examined in a spectroscope, or else modifies radiation which passes through it. In the second, some detectable property of the matter, or radiation from it, is changed appreciably by subjecting the sample to an electromagnetic field of relatively low frequency.

Examples of the first class include: emission, absorption and Raman spectroscopy, nuclear induction, and conventional microwave spectroscopy. Methods of the second category do not have almost universal applicability as do those of the first, but when appropriate, may afford resolution not otherwise attainable. Three of the four papers this afternoon deal with methods of the second class. Even in Purcell's report on radio astronomy the hydrogen hyperfine transition was first discovered with an atomic beam, one of the methods of the second class. In Deutsch's work on positronium a static or microwave magnetic field can trigger the emission of half million volt gamma rays.

I shall describe briefly the application of methods of the second type to a study of energy levels of the hydrogen atom. For simplicity of discussion I consider an idealized hydrogen atom in which the electron has no spin or magnetic moment, and relativity effects may be neglected. A consequence of the Coulomb attraction is that levels with the same principal quantum number n and differing azimuthal quantum numbers l should coincide in energy exactly. In actuality, the coincidence is not exact, and the frequency separations lie in the microwave and radio-frequency spectrum.

If we excite hydrogen atoms by electron bombardment, both levels ns and np will be populated. In general, the level ns is much longer lived than the corresponding level np. (For $n = 2$, $2s$ is metastable and has a life of a tenth of a second if the atom is isolated, while the life of $2p$ is about 10^{-9} second.) The life of $3s$ is about 0.1 micro-second and is 30 times longer than that of $3p$. The

cross-section is somewhat larger for the p level, but the longer life of s outweighs this, and one therefore expects to have a larger steady state population of ns than of np produced by the electron bombardment. The state $2p$ can radiate the Lyman-alpha ultra-violet radiation while $2s$ cannot because of selection rules. Hence if we cause transitions $2s$-$2p$ by r.f. fields of frequency corresponding to the energy separation of the states, we get an increase of intensity of Lyman-alpha or alternately a decrease in the number of metastable atoms. The former circumstance was used by Miriam Skinner and myself in a determination of the fine structure of singly ionized helium, and the latter by Retherford, Dayhoff and Triebwasser for a study of atomic hydrogen and deuterium.

The levels $n = 3$ can radiate both the Lyman-beta and H-alpha radiation of the Balmer series. While the level $3s$ can emit just the latter, only 10% of the $3p$ atoms can give the former because of the competing emission of Lyman radiation. The intensity of H-alpha should be decreased when radiowaves of the right frequency are applied to the excited atoms and cause transitions between $3s$ and $3p$. At Stanford University, Maiman and I are attempting a determination of the $n = 3$ fine structure in this way. Calculations of expected effects and preliminary observations are very encouraging.

Fairly obvious extensions of method should permit the study of higher levels of hydrogen and singly ionized helium. The only limit is sensitivity of the detector of the radiation and the amount of background light, which can usually be much reduced by a suitable filter.

When results of such measurements are compared with theory one obtains a test of the Dirac relativistic electron theory as well as the Coulomb inverse square law of attraction between nucleus and electron. For instance, in deuterium $2s$ was observed to be higher than $2p$ by 1059.0 Mc/sec. Most of this discrepancy has been explained by theoretical developments since 1947. About 1058.3 Mc/sec is caused by the interaction of the electron with the quantum mechanical vacuum (zero point oscillations of the electromagnetic field, and the infinite sea of negative energy electrons). This factor effectively smears out the point electron into a sphere of radius 7×10^{-12} cm, and in addition gives the electron an anomalous magnetic moment. About 0.75 Mc/sec is contributed by the

finite extension of the deuteron, while -0.6 Mc/sec is due to nuclear recoil effects.

When all such computations are added up there still seems to be a discrepancy of 0.5 Mc/sec, with claimed accuracy of about 0.1 Mc/sec for both experiment and calculations. The explanation is not known at present, but is probably to be found in some higher order quantum electrodynamic effect or in a short-range electron-proton interaction.

For distances of several feet or so, Coulomb's law has been tested very accurately by Faraday Ice Pail experiments. Suppose we were to consider a change from $1/r^2$ to $1/r^{2\pm\epsilon}$. From the negative result of such experiments, Maxwell was able to show that $\epsilon < 1/(21,600)$, while in 1936 Plimpton and Lawton found $\epsilon < 2 \times 10^{-9}$.

It is amusing to consider what value of ϵ would arise if we were to interpret the discrepancy of 0.5 Mc/sec as arising from such a change in the law of force for the much smaller distances of order 10^{-8} cm and less. One finds $\epsilon = 1 \times 10^{-8}$. In this sense then the microwave measurements of the hydrogen spectrum may be regarded as " Faraday Ice Pail " experiments, on a very small scale, in which a positive effect is found. However, as indicated earlier, it is certainly not claimed that any departure from Coulomb's law can be described by a simple change of the exponent from two.

Line Spectra in Radio Astronomy

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Over the range of wavelength from one centimeter to ten meters, roughly speaking, the earth's atmosphere is practically transparent to electromagnetic radiation. The long wavelength limit of this " radio window " is imposed by the ionosphere. At the short wavelength end, selective absorption by oxygen and water vapor is encountered, though bands of partial transparency may be found down to wavelengths of a millimeter or less. Most of the radio energy that reaches the earth from the outside, whether from the sun or more remote sources, is distributed continuously over the spectrum, like white light, but with an intensity that increases strongly toward *longer* wavelengths. It is convenient to describe the intensity of the radiation at a particular wavelength by giving the temperature of a cavity within which one would find the same flux of radiation at that particular wavelength. In the microwave band, the incoming radiation has so low an intensity that its " equivalent radiation temperature " is at most 10 or 20 degrees Kelvin. What radiation there is appears to come mainly from our own galaxy.

The possible existence of a characteristic spectral line in this weak continuum was suggested by the Dutch astrophysicist H. C. van de Hulst. A substantial fraction of the matter in our galaxy is atomic hydrogen, sparsely distributed as an interstellar gas. The ground level of the hydrogen atom is actually double, owing to the interaction of the magnetic moments of the electron and the hydrogen nucleus. This " hyperfine " splitting corresponds to a frequency of 1420 Mc/sec, so that the hydrogen atom in its ground state is capable of absorbing or emitting quanta of this frequency. The presence of this " 21 centimeter " line was first detected about two years ago by H. I. Ewen at Harvard. Very soon thereafter it was observed by Oort and Muller in Leiden, and by radioastronomers in Australia.

The method of observation is simple in principle. A microwave antenna is directed at the Milky Way and the energy received in

one narrow spectral band is carefully compared with that received
in some nearby spectral band; the spectrum is slowly explored in
this way, until one finds a narrow band in which the intensity is
significantly different from that in the neighboring parts of the
spectrum. The hydrogen line appears in emission; that is, the
intensity of radiation at this frequency is higher than that of the
continuum. The difference in equivalent radiation temperature
between line and continuum is only some 50 degrees, correspond-
ing to about 10^{-17} watts change in the input to the receiver. (The
hydrogen 21-cm radiation falling on the entire earth amounts to
about one watt!) Still, the refinement of radio techniques permits
one to measure not only the intensity of the line, but its shape or
"profile," and, of course, its frequency. Already much has been
learned about the distribution of hydrogen in the galaxy and the
conditions within the tenuous clouds of otherwise quite invisible
interstellar gas. For example, the measured line intensity is closely
related to the actual kinetic temperature of the hydrogen gas,
which in this way is found to be some 50 to 100 degrees Kelvin.

Are there sources of line emission other than the hydrogen
clouds? Are there other atomic or molecular transitions that might
be found in the radio spectrum of astronomical sources? One thinks
first of the sun, and here one meets a serious obstacle. The outer
solar envelope is an extremely hot gas, almost totally ionized. It
strongly emits *and strongly absorbs* radio waves. The cooler in-
terior layers near the photosphere, where unionized atoms can
exist, are screened from us by this opaque blanket of ionization.
There does not seem to be much hope of finding the 21-cm line in
the sun's radio spectrum; Oort and Muller in fact looked for it
without success. There is another interesting microwave line from
hydrogen, the transition $2S_{1/2} - 2P_{3/2}$ in the first excited state.
This is one of the transitions involved in Professor Lamb's experi-
ments. Were it not for the circumstance just mentioned, this line
(wavelength about 3 cm) ought to appear in the sun's spectrum,
and perhaps under special conditions it might still be found. As
for stars other than the sun, one must remember that the resolving
power of a radio telescope is so poor that a single remote star can
make only a relatively tiny contribution to the power entering the
receiver at one time. Except for local objects, one must think in
terms of extended, or at any rate multiple, sources.

Returning to the interstellar medium, the abundance of atoms other than hydrogen is of course very small. Deuterium has a hyperfine transition, the detection of which does not seem entirely hopeless. It has been suggested by the Russian astrophysicist Shklovsky that certain of the *hydrides*, in particular OH and CH, which are believed to be present in the interstellar gas, have lines in the microwave spectrum. These lines arise from the so-called Λ-type doublets of the ground state of the molecule. In the case of OH, this transition has recently been detected in the laboratory by Townes at Columbia, thus giving the radio astronomer the important advantage of knowing just where to look in the spectrum.

The microwave spectroscopists have, to be sure, catalogued a vast number of molecular rotational lines throughout the centimeter-wavelength range. But to have its pure rotational transitions fall in the centimeter range, a molecule must have two heavy ends, so to speak. Such molecules are very rare in a gas of which the overwhelmingly predominant constituent is hydrogen, at a total pressure of 10^{-19} atmospheres! Perhaps the most promising candidates are the heavy hydrides such as CaH, for which the first rotational transition occurs at a wavelength a little under one millimeter. But before radio astronomy can move into this range there will have to be some advance in microwave techniques. And we shall have to explore the atmospheric absorption to see if we can in fact find a reasonably transparent "window" through which to look out.

Summary of Communication
by Enrico Fermi on Meson Physics

WALTER SELOVE
Harvard University

Dr. Fermi discussed the development of meson physics, which was started by Yukawa's original theoretical suggestion in 1935 that the short-range forces between nucleons could be interpreted in terms of a field whose particles, the mesons, have a mass related to the range of the forces. The particles of approximately correct mass discovered shortly afterwards in cosmic radiation were shown some ten years later to consist of "mu"-mesons, which do not interact strongly with nucleons. There are, however, "pi"-mesons, which do seem to fit the specifications of Yukawa's theory, and most meson research at present is directed at their properties and interactions with nuclear matter.

The pi-mesons were also originally discovered in cosmic radiation. Cosmic radiation still is the only source of the highest energies. (Fermi pointed out that even the present accelerators, which produce particles with energies measured in billions of electron volts, cannot compete with the 10^{16} electron volt energies found in cosmic rays; to obtain such energies with present methods one would need a ring of satellites with a field of 10,000 gauss all around the earth — not a simple operation, but at least one would have no difficulty in obtaining the necessary vacuum!) Within their energy range, however, present accelerators furnish mesons in enormously greater numbers than those from cosmic rays; for example, at Chicago the 450 million volt proton beam produces a very respectable meson source of 10^{10} to 10^{11} mesons per second.

What kind of information can one hope and try to get about mesons? As for any new particle (and very many new ones are currently being discovered) one first asks questions on the particle itself — its charge, mass, spin, parity, etc. The charge ($+1$, -1, and zero are known), spin (zero), and parity (odd) have been determined through experiments, and the mass is at present known for the various types to about 1%. The next kind of investigation

we try to carry out with the mesons, following again a fairly well established pattern with regard to new particles, is to scatter them against various other particles and try to interpret the results in terms of the forces that operate between the mesons and the other particles. The possible kinds of scattering experiments are limited by the type of particles we can have in reasonably abundant concentration — e.g., it would be a rather unfair question to ask for a direct measurement of the scattering between neutrons and mesons because we cannot very well obtain a container of either neutrons or mesons with which to carry out the experiment. There is, however, a further and even more fundamental limitation to the information we can obtain in such scattering experiments. The interactions of the fundamental particles with the fields are so strong that each object that we call an elementary particle does not consist of an ideal mathematical point object, but instead it has a diffuse complicated structure, covering a region with a diameter of the order of 10^{-13} cm. A scattering between two such particles can thus be regarded as a kind of hydrodynamical collision, the results of which do not give us very direct information about the interactions of the ideal mathematical particles. But, we do get some information by such experiments, and this limited type of information is as much as we can hope to acquire by the experimentation that is in sight at present.

The present scattering experiments can be analyzed to give the dependence on angular momentum, on isotopic spin, and on energy. The experimental data is rather crude, because the scattering cross sections involved are so small that counting rates are very low. Within this limitation, the experimental data obtained so far in the scattering of negative and positive pi-mesons by protons may be described in a self-consistent way within the present framework of scattering theory, but it is too early yet to draw very detailed conclusions about the nature of the interactions.

Summary of Communication
by J. Robert Oppenheimer on
Recent Progress in the Understanding of
Nuclear Forces

WALTER SELOVE
Harvard University

Dr. Oppenheimer spoke on recent progress in understanding nuclear forces. We now know a good deal more about nuclear forces than we knew as recently as five to seven years ago, although the theory is still in a rather crude state. One might say that the three most important factors involved in an understanding of nuclear forces are (1) experimental information, (2) general principles, and (3) good ideas. With regard to the first of these, much new information has become available because of the higher energies available since the war. In the area of nucleon-nucleon interactions at moderate energies, these experiments have shown the neutron-proton interaction to give scattering which is strongly predominant in the forward and backward directions, thus showing a small probability of high momentum transfer, whereas the proton-proton interaction shows a much " harder " nature, with scattering at high energies being essentially insensitive to angle and energy.

With regard to the second factor, there has been new evidence that nucleon-nucleon interactions are charge-independent, i.e., that neutron-neutron, proton-proton, and neutron-proton forces are equal. This principle is more puzzling than any other general invariance or conservation law, especially so because its fundamental explanation is unclear and because it seems to be only approximately true rather than rigorously.

As for new fundamental ideas, there have really been none since Yukawa's original one. Recent theoretical work has consisted of attempts to work out the details of a meson theory in the face of the difficulties mentioned below. Meson theories have involved difficult and uncertain calculations from the start, and although the

discovery of pi-mesons gave new life to the Yukawa hypothesis, one still has several misgivings about the present form of meson theory. For one thing, the fact that each " physical " particle presumably consists of a diffuse and complicated structure involving more than just a single " ideal " particle makes it very difficult to analyze experimental interactions in terms of " ideal " particles. Secondly, although meson theories can be made to give some agreement with experiment, agreement can be obtained by means of various, mutually inconsistent, theories. And third, and most important, the present form of meson theory has a logically unsatisfactory basis. It is based on a weak-coupling type calculation, whereas in fact the interaction between nucleons and mesons is known to be strong.

This last difficulty may be compared with the situation in electromagnetic theory. First, as to the magnitude of the interaction constant — the number which measures the extent to which the " real " particle differs from the " ideal " particle, measures the extent to which interaction between the particle and the field complicates the situation. For the interaction between the electron and the radiation field, the interaction constant is about 0.01. For the quantum theory of the meson field, the corresponding number is of the order of 10. Now the interaction can be formulated in terms of a power series expansion in powers of the coupling constant. For the electromagnetic field one may hope that the successive terms in such a series will grow rapidly much smaller. Even for this field, however, there were serious difficulties in obtaining such a convergent type of result, until the recent development by Schwinger, Tomonaga, Feynman, and Dyson, on " re-normalization " of charge and mass. Since their work, the theory of quantum electrodynamics has been in a fairly satisfactory state. However, in the case of meson theory, an expansion in terms of a coupling constant of order 10 would certainly not be satisfactory. Actually, it appears that it may be possible to formulate the theory so as to involve an expansion not in powers of this coupling constant, but in the product of it with the square of the ratio of the meson and nucleon masses, a number about .01.

The big question in the present state of the theory is whether or not one can obtain a correct expansion solely in this smaller product parameter. It is not certain whether within this " weak-

coupling " framework there are convergent solutions at all. In any event probably the most one can hope for from this approach is a rough approximation to a correct description of nuclear forces.

Dyson has shown how one might carry out re-normalization in the meson field problem, and how one might hope to obtain a theory having perhaps some accuracy in the low-energy region. This approach is now being primarily used in the meson-nucleon scattering problem. A somewhat similar idea has been pursued by Levy in investigating the nucleon-nucleon interaction. He unfortunately made some serious errors in his work, but his results may nevertheless be important because of the good agreement obtained by comparing the results of his calculated nucleon-nucleon potential with experimental data on the properties of the neutron-proton and proton-proton systems at energies up to about 40 Mev. Many further calculations are being made along the lines of his approach. They give various forms for the interaction potential, although they all seem to have in common the short-range repulsive feature indicated by Levy. Results are not yet complete as to whether these newer attempts can actually give good agreement with experiment. It is at present not clear whether a theory along the lines of Yukawa's original idea can be made to work within the present formulation, or whether it will be necessary to introduce the very deep reforms in the whole description of the structure and interaction of elementary particles which we know must eventually be made.

Nuclear Spectroscopy

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The lecture gave a condensed summary of *experimental results and methods in nuclear spectroscopy developed at the Nobel Institute of Physics in Stockholm*, illustrated with several slides of the spectrometers and other instruments used in these investigations. By refinements in the design of the spectrometers, and improvements in the experimental techniques, the resolving power and the accuracy have been highly increased. Whereas a few years ago a precision of a few per cent was considered satisfactory, the accuracy now obtained is of the order of a few parts in 10,000. Such an increase in accuracy has in this case, as is the general experience in other fields of experimental physics, resulted in revealing many new facts.

For absolute determinations of β-γ energies the classical semicircular type of β-spectrometer is best suited. The new resonance method of Purcell has been used to measure the strength of the magnetic field, as well as to correct for the deviation from homogeneity in the field.

A spectrometer of the same type but using permanent magnets has been developed with the object of getting high resolving power. For this purpose it is necessary to have the spectrometer at constant temperature. A variation of one degree centigrade means a change of 2.2 parts in 10,000 in the magnetic field, which is of the same order of magnitude as some of the effects found and studied.

In the double-focusing spectrometer which was designed and first built in 1946, and later developed into a high precision instrument, the magnetic field is not homogeneous but varies as $1/\sqrt{r}$. In that case one gets a focussed true image in natural size of the β-radiating source at an angle of $\sim 255°$ from the object.

In addition to these three spectrometers with flat magnetic fields, some spectrometers of the lens type have been used in our investigations.

In one of these the β-rays are first collected in *an intermediate ring-shaped image* and then focussed on the G.M.-tube. The desired gradient in the magnetic field is obtained by adjusting the current in the different magnetizing coils. In this way high intensity can be combined with good resolving power. By the use of an adjustable ring-shaped screen in the intermediate focus, the resolving power can be varied within certain limits.

Finally we mention a new type of instrument, a "spectrogoniometer," where a combination of two lens spectrometers is used in the study of coincidence and angular correlations. The two spectrometers can be set at different angles to each other; the radiating source is placed in the center of rotation.

From the results we obtain a list of energy standards for use in β-spectroscopy. These standards cover at present a region of energies from 238 kev to 2753 kev (see Table I) with an accuracy of a few parts in 10,000. In the few cases where measurements have been carried out in other laboratories, especially with the crystal method, at DuMond's Institute in Pasadena, the agreement is now quite satisfactory.

TABLE I

Line	H_ρ Gauss cm	E kev	Method
ThB F	1388.56 ± 0.15	238.63 ± 0.04	absolute
ThB I	1754.01 ± 0.20	238.62 ± 0.04	absolute
Au^{198} K conv.	2222.4 ± 0.4	411.75 ± 0.10	against ThC"L
ThC" L	2607.17 ± 0.30	510.85 ± 0.08	absolute
Cs^{137} K conv.	3381.28 ± 0.5	661.65 ± 0.15	absolute, against ThC"L, against annihilation rad.
RaC' R	4839.8 ± 0.8	1120.4 ± 0.2	absolute
Co^{60} I		1172.8 ± 0.5	against Co^{60} II
Co^{60} II		1332.5 ± 0.3	against RaC'T
RaC' T	5874.4 ± 0.6	1415.8 ± 0.2	absolute
ThC" X	9986.7 ± 1.5	2614.3 ± 0.5	absolute
$Na^{24}U_K$	10363 ± 4	2753.2 ± 1.0	against ThC"X

As examples of what can be gained by the increased resolving power of the spectrometers, there is shown in Figure 1 spectrograms of some lines in the spectra of Th B + C + C". The F, G, and H-lines, which originate from the K-shell, are definitely broader than the I and I_a-lines belonging to the L_I and L_{II} shells.

ThB + C + C″
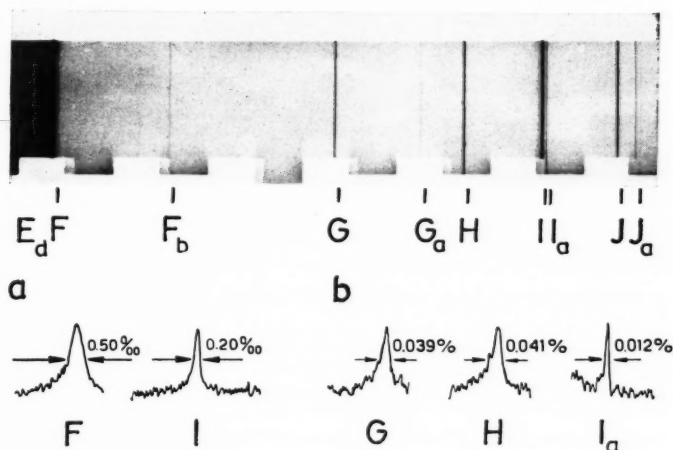
FIGURE 1. *Upper half:* β-SPECTROGRAM TAKEN WITH SEMICIRCULAR, PERMANENT MAGNET SPECTROMETER FOR HIGH RESOLUTION. *Lower half:* PHOTOREGISTRATIONS FROM DIFFERENT PLATES: a, OF THE LINES F AND I; AND b, OF LINES G, H AND I_a. (SLÄTIS)

respectively, the difference in width being about 3 in 10,000. This effect can be quantitatively explained in terms of the finite widths of the K level, as extrapolated from the X-ray data for lighter elements.

As another example, the successful elucidation of the very complicated internal conversion spectrum of Bi^{206} is shown in Figure 2, taken from an unpublished paper by Dr. Alburger. The results from the successive increases in resolving power are illustrated. By this investigation it has been possible to give a tentative energy level scheme, which fits the observed data. In part this scheme was based on coincidence measurements by using the spectrogoniometer.

A PORTION OF THE Bi[206] (K) Pb[206]
INTERNAL CONVERSION SPECTRUM
(D.E. ALBURGER - UNPUBLISHED DATA)

DOUBLE - FOCUSING SPECTROMETER
AT 0.22 % RESOLUTION

K-881

K-895

L-803

M-803

DOUBLE - FOCUSING SPECTROMETER
AT 0.5 % RESOLUTION

LENS SPECTROMETER (BROOKHAVEN)
AT 1.2 % RESOLUTION

3950   4000   4050   4100   GAUSS- cm
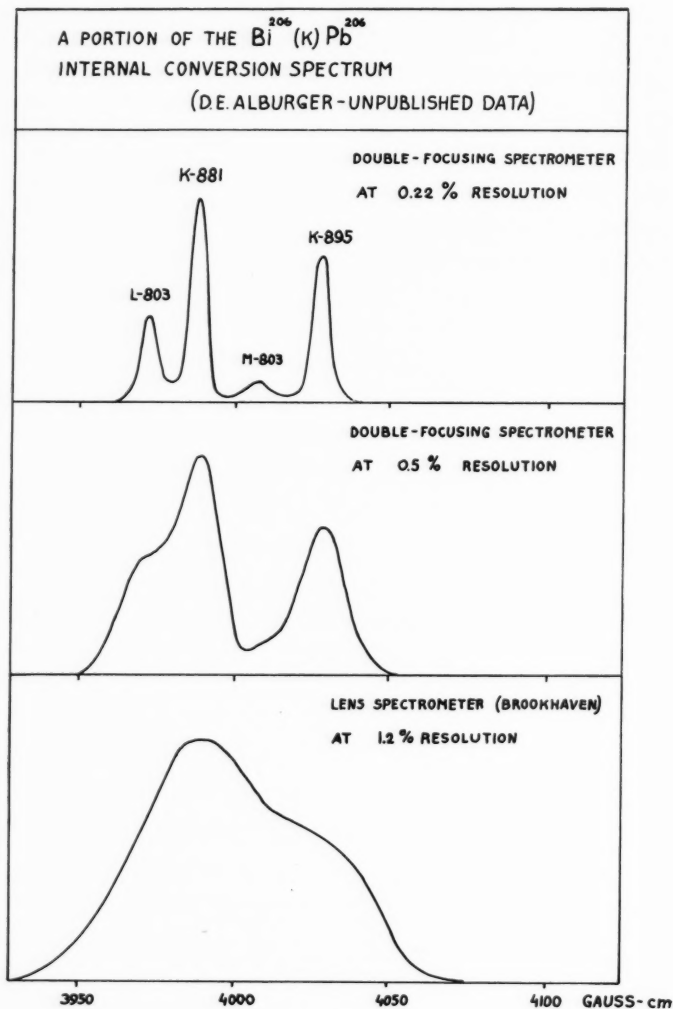
FIGURE 2

The foregoing refers mainly to researches reported in the following papers, and to some newer researches by Dr. Alburger not yet published.

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Phase Transitions in Atomic Nuclei

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Recent advances in our knowledge of the properties of complex nuclei have revealed a number of new and unexpected features. One of the outstanding surprises that have startled nuclear physicists in recent years is the success of the shell model of nuclear structure. This model assumes that the nuclear constituents (nucleons) can be considered to move with very little interaction among each other in a potential well. This potential well represents the overall effect of all other nucleons.

The most unexpected feature of this model is the existence of nucleon orbits within the nucleus with well-defined energy and angular momentum undisturbed by the other nucleons. This is in contradiction to the concept of a strong interaction between nucleons at distances such as those which occur in nuclear matter. No satisfactory explanation has yet been proposed for this phenomenon. Either the nuclear forces are different within nuclear matter compared to the forces between isolated nucleons, or some special mechanism or specially ordered arrangement in the low excited states causes the particles to move as if they were free within a potential well.

Phenomenologically, we can express this fact by the introduction of a " mean free path " l in nuclear matter for the exchange of energy or momentum with other nucleons. The success of the shell model implies that this mean free path l is long compared to the nuclear radius for low excitation energies. The question arises whether l changes with higher excitation energies. There are a number of facts which lead one to believe that l becomes appreciably shorter for higher excitation. The large amount of experimental material concerning nuclear reactions suggests that a compound nucleus is formed after the bombarding of a nucleus with nucleons of several Mev. This means that these projectiles have a mean free path smaller than the nuclear radius. The reaction cross section (cross section for energy exchange) for

neutrons at 15 Mev actually is rather accurately equal to the geometrical cross section ($\pi(R + \lambda)^2$, λ being the deBroglie wavelength). This indicates compound-nucleus formation for every hit and thus $l << R$. Recent investigations of the total neutron cross section between 0 and 3 Mev indicate that in that region $l \sim 3R$.[1] At very high energy (50 Mev and above), the observed interaction cross section between isolated nucleons becomes small. This gives rise to an increase[2] of l towards $l \sim R$. At these high energies, however, the large l is easily understood from the decrease of the n-p and p-p cross section, whereas the large l at very low energies is yet unexplained. We concentrate our attention on the change in character of the nucleon-nucleus interaction from weak to strong, which seems to take place somewhere at an excitation energy of the order of 8 or 10 Mev.

A similar change can be observed in the level density. Spectroscopic results and inelastic scattering show that the level spectrum of complex nuclei is of the character shown in Figure 1. It has
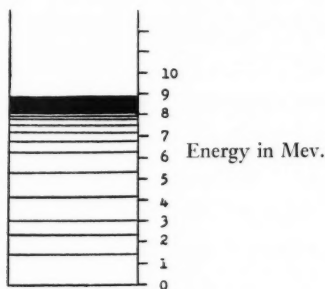
FIGURE 1. TYPICAL LEVEL SPECTRUM OF A COMPLEX NUCLEUS.

large spacings and is similar to a single particle spectrum (shell model) at lower energies, perhaps up to 5-7 Mev. We know from neutron capture experiments, however, that at 8 Mev and higher the levels are very dense. In nuclei with $A > 100$ the level distance is of the order of 30 ev, that is, the levels are about 10^4 times denser than in the lower region. There seems to be a qualitative difference at low and at high excitation which one can interpret as the

transition from the lower well-ordered states of independent par-
ticle motion to the higher states with strong interaction in which
the motion is complicated and shared by all constituents. Hence
the energy dependence of the level density also reproduces the
two characteristic nuclear behavior patterns: At low energies the
motion is relatively simple, each nucleon moves in an apparent
potential field, the stationary states are few and widely spaced.
At higher excitation the motion is much more complicated, collec-
tive modes of motion are possible, the nucleons exchange energy
and momentum, the energy is thoroughly mixed among the con-
stituents, the levels are many and narrowly spaced.

The sharing of the energy at higher excitation among many
constituents is well illustrated by the energy distribution of in-
elastically scattered neutrons of 14 Mev.[3] The secondary neutrons
all have an energy much smaller than the primary energy (about
1 or 2 Mev). This means that the energy very probably is dis-
tributed over many partners in the nucleus. The spectrum of the
emerging neutrons can be compared to the energy distribution in
an evaporation. Actually one can derive a temperature from the
observed energy distributions, and this temperature was found to
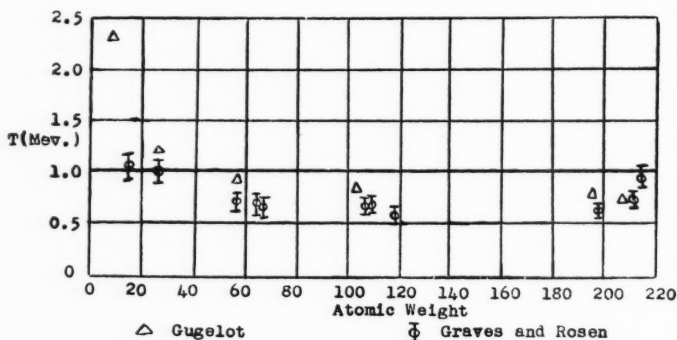
FIGURE 2. VARIATION OF TEMPERATURE WITH MASS NUMBER.

be quite low (~0.75 Mev) for most nuclei. Figure 2 shows the
temperatures of the outgoing neutrons observed after bombarding
with 16 Mev protons[4] and 14 Mev neutrons.

In terms of the evaporation model it is somewhat surprising that the temperatures of all nuclei above $A \sim 30$ are found roughly equal. One would think that the same energy (15 Mev) would heat up lighter nuclei to a higher temperature than heavier ones simply on the basis of equipartition of energy.

An understanding of this fact is perhaps suggested by the use of simple thermodynamic concepts, whose application to nuclear problems, however, is highly questionable. The two types of states in nuclear matter which we encountered at low and high excitation might perhaps be interpreted as two " phases " of nuclear matter; one ordered phase of low entropy and low level density and another phase of higher entropy and level density, which corresponds to the statistical motion. A certain amount of latent heat is necessary to " melt " the first phase into the second. The 15 Mev which are delivered to the nucleus are probably sufficient to " melt " a nucleus smaller than $A \sim 50$. Hence, the temperature which is obtained rises with decreasing A for $A < 50$. If $A > 50$, the energy is sufficient to " melt " only part of the nucleus. Hence, the temperature that is reached is independent of A and is equal to the " melting " temperature (probably ~ 0.75 Mev). Even at the highest A the energy is enough to melt part of the nucleus.

The applicability of this qualitative picture might be tested by further experiments at higher and lower energy. At somewhat higher incident energies the temperatures of the heavier nuclei should be again about equal to the " melting " temperature, but the limiting A for which this occurs should be correspondingly higher. Further experiments will tell whether it is possible to describe the qualitative behavior of the emitted nucleons by applying thermodynamic analogies. Such concepts are sometimes useful for a first orientation and can serve as a valuable guide for the sifting of the complicated experimental material.

The study of the behavior of complex nuclei is of great importance for the fundamental question: How do nuclear particles interact when they occur in large aggregates? We are faced with a quite different problem here than, say, in the structure of solids. In the latter case, we do know the elementary forces acting between atoms, and the problem is entirely a mathematical and methodical one of applying this knowledge to an aggregate of atoms. In the nuclear case, however, the elementary forces be-

tween the constituents are almost completely unknown, since the little knowledge which we have acquired from the study of the forces between two isolated nucleons seems to be inapplicable in the case of densely packed nuclear matter. The question of what keeps neutrons and protons together in atomic nuclei is still largely unanswered.

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